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# STUDIES OF THE EFFECT OF CHANGE OF DRIVE: II. FROM HUNGER TO DIFFERENT INTENSITIES OF A THIRST DRIVE IN A T-MAZE

HOWARD H. KENDLER, SEYMOUR LEVINE, EDWARD ALTCHEK, AND HAROLD PETERS[1]

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In the first study of this series (1), it was found that after the acquisition of a turning habit in a T-maze under hunger motivation and dry food reward the invocation of a thirst drive would cause some animals to "switch" their spatial response, i.e., turn left if the turning habit acquired under hunger was to the right. The proposed explanation of this switching phenomenon was that when *Ss* were hungry and dry food was in one of the end boxes, the stimuli emanating from the food box, and the alley leading to the food, became associated with fractional anticipatory eating responses. The cues produced by these anticipatory responses became themselves, in turn, conditioned to entering and continuing locomotion in the alley leading to the food box. During the early stages of learning only the hunger drive stimulus and the dry food would evoke the anticipatory eating responses. As the learning to go to food progressed, however, these anticipatory responses became conditioned to certain features of the goal box containing food, and then "worked backwards" until stimuli at the entrance to the correct alley were capable of evoking some component of the eating response. When *Ss* were made thirsty, the stimuli of the maze evoked these fractional anticipatory eating responses. It was assumed that the anticipatory eating responses of dry food are, in some manner, incompatible with the resultant condition of water deprivation (probably conflicts with the anticipatory drinking responses instigated by intense thirst drive stimuli). It would be expected that this "incompatibility" at the choice point would be reduced by *Ss* going in a direction away from the end box baited with food. The cues in the incorrect path of the maze had not been directly conditioned to the anticipatory eating responses and consequently the greater the penetration into this path, the greater would be the relief from the "conflict state" postulated to exist.

The above theoretical analysis generates the deduction that any factor

[1] The senior author is responsible for both the article in its present form and for the design of the experiment. S. L. obtained data for one study, and supervised E. A. and H. P. in the collection of the data for the other two replications.

which would intensify the conflict between the anticipatory eating response at the choice point and the resultant condition of water deprivation should increase the amount of switching. This intensification of the conflict could be accomplished by increasing either the strength of the anticipatory eating response or the strength of the condition resulting from water deprivation.

In the first study (1), the above theoretical analysis was tested by manipulating the strength of the anticipatory eating response by varying the amount of practice to the food side. It would be expected that the more an animal overlearns to go to one side of a T-maze to get food, the stronger will be its anticipatory goal responses at the choice point and consequently the greater should be its tendency to switch when its motivation is shifted to thirst. This prediction was confirmed (1); *S*s required to reach a criterion of 12 successive correct choices evidenced 48% switching while only 30% of those *S*s who reached a criterion of three successive correct responses switched.[2]

The present experiment is concerned with testing another prediction. It would be expected that following training sufficient to produce a fairly strong anticipatory eating response at the choice point, a shift to a strong thirst drive should produce more switching than a shift to a weak thirst drive. This prediction would follow because a shift to a strong thirst drive should increase the tendency *to avoid* the cues evoking the incompatible anticipatory eating responses.

[2] It is possible that with increasing amounts of overlearning to go to the food side, a point might be reached beyond which additional training would result in a decrease in the per cent of switching. This would occur if the cues at the choice point were capable of acquiring a sufficiently strong habit strength to overcome the tendency of the animals to switch. Levine (2) recently found, however, that a shift to a 22-hr. thirst drive after 90 reinforcing trials to the food side produced 63% switching. The mean number of reinforcements required by the four groups of *S*s in the Kendler and Levine study to reach the criterion of 12 successive correct choices ranged from 17 to 25 reinforcements and, as is indicated above, they showed an average of 48% switching.

## Procedure

The experimental procedure of the three replicated studies employed in this experiment is essentially the same as that used for the third and fourth studies of the initial experiment (1), except for the modification required to test the above prediction. Since the procedure is described in detail elsewhere (1), only a brief description is presented here.

The *S*s for this experiment were 79 naive male albino rats, of the Wistar strain, purchased from the Albino Farms, Red Bank, New Jersey. Their ages ranged from 70 to 95 days at the beginning of the experiment.

The apparatus was a single choice T-maze. After preliminary training, all *S*s were given three trials daily following 22 hr. of food deprivation. Food in the form of 15 pellets of Purina Laboratory Chow was present in one of the end boxes, while the other end box remained empty. All *S*s were trained until they reached the criterion of four days of correct choices (at least 12 successive errorless trials).

On the test trial which followed the last criterion trial by approximately 24 hr., half of the *S*s were motivated by a weak thirst drive (3 hr. of water deprivation) while the remaining half were motivated by a strong thirst drive (22 hr. of water deprivation). Group 3 was given 5 g. of food and permitted to drink for 1 hr. following the last criterion trial. Following the termination of the 1-hr. drinking period, an additional 5 g. of food was presented to *S*. Four hours prior to the test trial the water bottle was reinserted into the cage and *S* was permitted water for 1 hr. One hour prior to the test trial a dish of kibble meal was presented to *S*s in order to produce as complete food satiation as possible. The same procedure prevailed for Group 22, except that no water was available for 22 hr. prior to the test trial.

## Results and Discussion

Table 1 presents the major results of this experiment. In all three studies it was found that *S*s whose drive state was shifted to a strong thirst drive demonstrated more switching than *S*s whose motivation was shifted to a weak thirst drive. Since all studies utilized the same experimental procedure, it appears justified

TABLE 1

NUMBER AND PER CENT OF *Ss* SWITCHING THEIR SPATIAL RESPONSE FOLLOWING SHIFT OF MOTIVATION FROM HUNGER TO THIRST

| Study | Group 3 | | | Group 22 | | |
|---|---|---|---|---|---|---|
| | *N* | Switching | | *N* | Switching | |
| | | *N* | % | | *N* | % |
| 1 | 20 | 4 | 20.0 | 21 | 10 | 47.6 |
| 2 | 9 | 2 | 22.2 | 9 | 5 | 55.6 |
| 3 | 10 | 2 | 20.0 | 10 | 3 | 30.0 |
| Total | 39 | 8 | 20.5 | 40 | 18 | 45.0 |

and advantageous to combine the data. By combining the data of the three studies, we obtain a $t$ of 2.38, which is significant, according to our one-tail hypothesis, beyond the 1% level.

The significance of these results is twofold. Firstly, they once again demonstrate (1), on the empirical level, the importance of drive stimuli in habit formation and habit utilization. Too often in our discussion of habits of an organism, we tend to disregard the motivational conditions under which they were acquired. The present study testifies to the importance of the drive stimulus component of the habit structure and demonstrates the influence the intensity of a "new drive" has upon the expression of an "old habit." Secondly, on the theoretical level, the present experiment demonstrates the operational meaningfulness, as well as explanatory capacity, of the concept of fractional anticipatory goal response. The significant test of a theory is not how well it can explain one set of data, but rather how well it can integrate economically different sets of data. Results contained in this paper, as well as those presented in the first paper of this series, add another set of data which appears capable of integration within the S–R reinforcement theory by means of the concept of fractional anticipatory goal response. Only through the systematic application of basic assumptions in different experimental settings can a generalized behavior theory be validated.

## SUMMARY

Following the acquisition of a moderately strong turning response in a T-maze under hunger motivation and dry food reward, the motivation of *Ss* was shifted to a weak or a strong thirst drive. The *Ss* who were shifted to a strong thirst drive evidenced a significantly greater amount of "switching" behavior, i.e., choosing the nonfood side. The data were interpreted in support of an avoidance hypothesis based upon the mechanism of the fractional anticipatory goal response.

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# THE ROLE OF THE VESTIBULAR ORGANS IN SPACE ORIENTATION

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The sensory basis of space orientation in man and animals has long been a matter of speculation and controversy. Mystic forces and new senses have been assumed to account for the capacity of animals to return to and recognize their places of residence. Almost every sense organ, at one time or another, has been thought to be important or necessary in space orientation. All too often conclusions are based upon the casual descriptions of the remarkable ability of animals to travel over long distances or upon the experimental removal of sense organs. The complex nature of spatial adjustments and the ability of the organism to compensate for sensory deprivations have been ignored in the simple and limited experimental investigations. Griffin (3), in a review of the experimental literature on the sensory basis of bird navigation, presents evidence for and against three types of theory: (*a*) visual cue theory, that is, that the bird spirals or explores the area until some familiar landmark is found; (*b*) kinesthetic cue theory, which states that kinesthetic cues are obtained while the bird is taken to the release point; and (*c*) the magnetic or electrical reception theory, which assumes that the bird is sensitive to the earth's magnetic field. He favors an explanation of homing based upon visual cues.

The importance of visual sensitivity in human orientation is demonstrated by the faltering and inefficient movements of the normal-sighted individual finding his way and avoiding obstacles in the dark. We become so accustomed to visual cues that we overlook the importance and contributions of other sense organs. Deprived of sight the blind man utilizes auditory cues in making his way about in total darkness (8, 10). Yet, visual or auditory stimuli can only provide us a frame of reference for space orientation; they do not give us directional knowledge. Our awareness of the experiences derived from distance receptors (visual, auditory, and olfactory organs) leads us to neglect the role of postural cues in directional ability, and yet fundamental to the development of space orientation is the integration of distance and postural mechanisms. Gregg (2), in a study on motor accompaniments in orientational perception, notes that consciousness of direction is intimately interlocked with actual sensations which are chiefly, if not exclusively, kinesthetic.

Among these postural mechanisms the vestibular organs have been ascribed a predominant role. Wundt (13) ascribes to the organs of the inner ear fundamental importance in orientation. He quotes the studies on the partial and total extirpations on the semicircular canals, especially on the canals of birds. Further confirmation he finds from the observation that deaf mutes frequently suffer from disturbances in orientation.

On the other hand, Jackson and Jampolsky deny the primary importance of vestibular functioning in space orientation. Jackson (4) notes that the vestibular function no longer holds the commanding position in aviation examination. Vision makes the most general and important contributions to orientation and equilibrium. He gives many illustrations of failure of coordination in the dark which is immediately rectified when the light is turned on. Jampolsky (5) admits that the perception of visual space is not wholly dependent upon vision for its determination. When one experiences an object as localized in space, one has labyrinthine, kinesthetic, and tactile cues in addition to the retinal image. These nonvisual cues alone, he maintains, are rather ineffective; the individual relies chiefly upon visual experience. His conclusions are based upon the results of experiments in which *S*s tried to localize points in space.

Studies on the role of the vestibular organs in space orientation, however, have been chiefly based upon casual observations and speculations. Experiments have been confined primarily to animals. It is difficult to justify

TABLE 1
DATA ON DEAF *Ss* USED IN THE FOUR EXPERIMENTAL GROUPS

| Group | Sex | | Hearing Loss | | Age at Deafness | | Total |
|---|---|---|---|---|---|---|---|
| | M | F | Total | Partial | Congen. | Advent. | |
| I Rotation+ Standing+ | 5 | 11 | 8 | 8 | 8 | 8 | 16 |
| II Rotation+ Standing− | 12 | 5 | 7 | 10 | 9 | 8 | 17 |
| III Rotation− Standing+ | 5 | 5 | 5 | 5 | 5 | 5 | 10 |
| IV Rotation− Standing− | 9 | 7 | 8 | 8 | 8 | 8 | 16 |
| Total | 31 | 28 | 28 | 31 | 30 | 29 | 59 |

the removal of the vestibular organs in human *Ss* for experimental purposes. Fortunately, however, nature has provided such *Ss*. Because of the proximity of the auditory and vestibular structures and the continuity of the membranous labyrinth of the inner ear, *Ss* suffering deficiencies in hearing, adventitious or congenital, frequently show vestibular defects. Worchel and Dallenbach (11, 12) have provided tests for determining the presence or absence of vestibular function. By systematic examination of deaf people we may find a sufficient number of *Ss* with varying degrees of vestibular sensitivity in order to determine the role of the vestibular organs in space orientation.

## METHOD

*Subjects.*—The *Ss* were selected from the Texas State School for the Deaf.[1] Only those *Ss* who were above the age of 12 yr., had the equivalent of a third-grade education, and had at least an IQ of 90 were used. Those who did not meet the requirements were not able to comprehend the space orientation tasks. The vestibular and audiometric tests were administered to 87 *Ss*. The vestibular tests of Worchel and Dallenbach (11, 12) consist of a series of standing and rotation tests. These tests allow us to determine the sensitivity of the semicircular canals and vestibular organs separately. Thus four groups of *Ss* can be isolated for study: Group I, rotation (+) and standing (+); Group II, rotation (+) and standing (−); Group III, rotation (−) and standing (+); and Group IV, rotation (−) and standing (−). We attempted to get enough *Ss* to complete a 2 × 2 × 4 factorial design: partial vs. congenital deafness, congenital vs. adventitious deafness, and the four degrees of vestibular sensitivity. We could not find, however, sufficient *Ss* in Groups III (17 *Ss*) and IV (10 *Ss*). Thus we aimed at a 2 × 2 × 2 factorial design: total vs. partial deafness, total vestibular sensitivity (I) vs. total vestibular insensitivity (IV).

More *Ss* were found in Group I than were needed for a 4-replication design. Most of them were in the congenitally deaf and partial-hearing columns. The eight selected for these columns in order to complete the design were picked at random from the ones tested. In Group IV were found more *Ss* in the totally and adventitious deaf column. This is probably due to the fact that congenital deafness probably arises from central nervous system defects which would not necessarily involve the vestibular organs or result in total deafness. Table 1 shows the 59 *Ss* finally selected for the present study.

*Procedure.*—The procedure in the present experiment followed that of Exp. C in the Worchel study (9) on space orientation in the blind. We could thus compare the results of the present study on vestibular cues with those on visual cues in space orientation in order to determine the relative importance of each of these sense organs in spatial tasks.

The experiment took place on a large outdoor rectangular concrete tennis court, 40 × 80 ft.,

[1] The cooperation of Mr. Roy M. Stelle, Superintendent, and Mr. O. L. MacIntyre, Director of Education, at the Texas School for the Deaf, in the selection of *Ss* and for providing experimental space is gratefully acknowledged.

TABLE 2

MEAN ORIENTATION SCORES (IN FEET) ON TESTS 1 AND 2 FOR THE DEAF ACCORDING TO DEGREE OF VESTIBULAR SENSITIVITY AND FOR BLIND AND NORMAL *Ss**

| Group | *N* | Test 1 | | Test 2 | |
|---|---|---|---|---|---|
| | | Mean | *SD* | Mean | *SD* |
| Deaf—I | 16 | 6.71 | 2.66 | 6.68 | 2.20 |
| Deaf—II | 17 | 6.39 | 2.14 | 7.22 | 2.84 |
| Deaf—III | 10 | 5.39 | .98 | 6.92 | 3.27 |
| Deaf—IV | 16 | 4.38 | 1.26 | 4.99 | 2.32 |
| Blind | 37 | 6.30 | 2.76 | 6.78 | 2.61 |
| Normal | 33 | 4.66 | 1.71 | 4.99 | 2.32 |

* Data for the blind and normal *Ss* taken from a previous study by Worchel (12).

located about 100 ft. from the main school building of the Texas State School for the Deaf. The nearest object to this area was a large oak tree that stood about 20 ft. from the north side of the area. We marked on the concrete the corners of eight isosceles triangles with the following hypotenuse dimensions: 8, 10, 12, 14, 16, 18, 20, and 22 ft. Two series of spatial problems were presented to each deaf *S*. Test 1 consisted in leading *S* along the two sides of the triangle. He was to return without guidance via the hypotenuse path to the starting point. In Test 2, *S* was led along the hypotenuse. Then he was to return to the starting point along two paths which formed the sides of the triangle. In each trial, the distance *S* stopped from the starting point was recorded to the nearest foot. As in the previous experiment (9), angular measurements were omitted since they were found to be highly correlated with the distance scores.

For Test 1, *S* was brought to the experimental area and a card containing the instructions was handed to him. The blindfold was then placed around *S*'s eyes. He was led to the starting point of the first triangle, tapped on the back, then led along the legs of the triangle. At the end of this path, he was told that hereafter he would have to return to the starting point in a straight path without guidance but *E* would show him how to do it in this trial so that he would understand. In the following trials, after *S* had been led along the two paths, *E* would say, "Now return to the starting place directly in a straight path like I showed you, and then stop." The order of the trials according to the hypotenuse distances was: 10, 14, 18, 20, 16, 12, 8, and 22 ft.

For Test 2, the instructions informed *S* that he was going to be led in a straight line and he was to return to the starting point by such a path that he would make a right angle before his return to the initial position. Again *E* showed *S* what to do by leading him along the hypotenuse of one of the triangles, pausing, and then returning to the starting point along the two legs of the isosceles right triangle. The order of the trials in Test 2 was the same as that in Test 1.

## RESULTS

The mean scores and *SD*'s (in feet) on Tests 1 and 2 for all four groups appear in Table 2. On Test 1, the means for Groups III and IV are smaller than the means for Groups I and II. Since Groups III and IV lacked semicircular canal functioning, it seems that the absence of semicircular canal sensitivity makes for better results on Test 1. On Test 2, the differences, though in the same direction, are not as great. Groups III and IV are also much more homogeneous on Test 1 than the other groups. The *SD*'s are .98 and 1.26, respectively, whereas for Groups I and II the *SD*'s are 2.66 and 2.14, respectively. The variability on Test 2 was much greater than that on Test 1. The *t* ratio between Groups II and IV is significant at the 1% level, and between Groups III and IV at the 5% level of confidence in Test 1. Between Groups I and III, and Groups II and III, though the differences are not significant at the 5% level, they were very suggestive. Between Groups I and III, the *t* ratio yielded a probability of about 7% and between II and III close to 10%. The *t* ratios for the differences between the means on Orientation Test 2 were not calculated since the analysis of variance yielded an F ratio of only 1.74 which is not significant at the 5% level. The results, though not entirely conclusive, indicate that the differences on the Orientation Test 1 are due to the semicircular canals, and not to the utricular-saccular organs. *Ss* with functioning

TABLE 3

ORIENTATION SCORES (IN FEET) ON TEST 1 AND TEST 2 FOR EACH *S* IN THE 2 × 2 × 2 FACTORIAL DESIGN

| *S* | Total Deafness | | | | Partial Deafness | | | |
|---|---|---|---|---|---|---|---|---|
| | Congenital | | Adventitious | | Congenital | | Adventitious | |
| | Vest. (+) | Vest. (−) | Vest. (+) | Vest. (−) | Vest. (+) | Vest. (−) | Vest. (+) | Vest. (−) |
| | | | | *Test 1* | | | | |
| 1 | 6.4 | 4.3 | 5.1 | 6.8 | 8.3 | 6.7 | 4.9 | 4.5 |
| 2 | 4.8 | 5.4 | 10.5 | 4.0 | 4.8 | 3.3 | 8.3 | 4.5 |
| 3 | 5.8 | 4.8 | 4.9 | 4.8 | 9.9 | 2.8 | 7.3 | 3.5 |
| 4 | 5.8 | 4.8 | 6.8 | 1.6 | 12.0 | 4.3 | 4.6 | 4.0 |
| Mean | 5.7 | 4.8 | 6.8 | 4.3 | 8.8 | 4.3 | 6.3 | 4.1 |
| $SD^2$ | .33 | .15 | 5.05 | 3.47 | 6.93 | 2.25 | 2.46 | .17 |
| | | | | *Test 2* | | | | |
| 1 | 7.0 | 6.2 | 5.0 | 6.5 | 6.6 | 4.7 | 5.3 | 4.5 |
| 2 | 3.0 | 5.8 | 7.9 | 4.5 | 4.5 | 3.0 | 8.0 | 4.3 |
| 3 | 4.6 | 5.5 | 6.8 | 4.2 | 11.8 | 5.4 | 5.6 | 4.6 |
| 4 | 9.5 | 5.9 | 6.0 | 2.3 | 10.1 | 2.4 | 5.3 | 10.0 |
| Mean | 6.0 | 5.9 | 6.4 | 4.4 | 8.3 | 3.9 | 6.1 | 5.9 |
| $SD^2$ | 6.55 | .65 | 1.13 | 2.22 | 8.21 | 1.49 | 1.29 | 5.76 |

semicircular did more poorly on the average on Orientation Test 1 than those with insensitive semicircular canals. On Test 2, however, the degree of vestibular sensitivity does not affect significantly the ability of the *S*s to return to the initial position. Table 2 also contains the data for the blind and normal *S*s used by Worchel in a previous study (9) under conditions similar to the present experiment. Comparisons with these *S*s allow us to specify the relative contributions of audition, vision, and vestibular sensitivity in space orientation. The difference between the means of the normal and deaf of Group IV on both Tests 1 and 2 is insignificant at the 5% level of confidence, but the difference between the means of Group I deaf *S*s and the normal is significant at the 1% level. These differences also hold true between Group II *S*s and the normal on both Tests 1 and 2. The difference between the means of the blind and deaf of Group I is not significant at the 5% level on Tests 1 and 2, but the blind were significantly inferior (1% level) to the deaf of Group IV.

TABLE 4

ANALYSIS OF VARIANCE OF THE $\sqrt{X + .5}$ TRANSFORMED TEST 1 SCORES

| Source of Variation | Sum of Squares | *df* | *F* |
|---|---|---|---|
| Between degrees of deafness | .06 | 1 | |
| Between etiology | .08 | 1 | |
| Between vestibular sensitivity | 2.00 | 1 | 15.38 |
| Deafness × etiology | .04 | 1 | |
| Deafness × sensitivity | .30 | 1 | 2.31 |
| Etiology × sensitivity | 0.00 | 1 | |
| Deafness × sensitivity × etiology | .15 | 1 | 1.15 |
| Within groups | 3.19 | 24 | |
| Total | 5.82 | 31 | |

The factorial design and the data on Test 1 are given in Table 3. Although the between-groups differences are significant at the 5% level ($F = 2.98$, for 7 and 24 *df*), Bartlett's test for homogeneity of variance yields a corrected chi square of 16.08, which is also significant at the 5% level. A transformation of the data ($\sqrt{X + .5}$) was used to reduce heterogeneity of variance. After this transformation the means and variances were no longer proportional and the heterogeneity of variances was markedly reduced. The complete analysis of variance on the transformed scores for the eight groups of Table 3 is shown in Table 4. For 1 and 24 *df*, an $F$ of 4.26 is significant at the 5% level, and an $F$ of 7.82 is significant at the 1% level.

The $F$ of 15.38 for vestibular sensitivity is clearly significant beyond the 1% level. An examination of the data in Table 3 shows that those *S*s who were insensitive to rotation and standing did significantly better than those who were sensitive.

The lack of significance of the other major variables and of the interactions indicates that the degree of deafness and etiology (congenital or adventitious) are not significant factors in the ability of *S* to return to the initial position of Test 1.

The scores on Test 2 (Table 3) also had to be transformed by $\sqrt{X + .5}$. Bartlett's test for homogeneity of variance yielded a corrected chi square of 14.95, which for 7 *df* is significant at the 5% level. The results of the two-part analysis of variance of the transformed scores gives an $F$ of 1.73 which is not significant at the 5% level. Therefore the hypothesis of random sampling from a common normal population cannot be rejected. Vestibular sensitivity, degree of deafness, and etiology of deafness do not significantly affect the ability of our *S*s in returning to the initial position on Test 2.

For all 59 *S*s, the product-moment correlation between Tests 1 and 2 is +.45, a moderate though significant correlation. Though there is some overlapping, the tests are measuring different abilities. The correlation here is much greater than the one obtained on the Worchel study of the blind (9). The correlation between Test 1 and 2 there was only .13.

There is no significant relationship between the chronological ages of our *S*s and the orientation scores on Tests 1 and 2. These results agree fairly well with those found on the blind (9). The correlations there were −.12 and +.26, respectively. Sex was not a factor in the performances on the orientation tests. The $t$ ratio between the means for the men and the women on Test 1 was 1.54, and on Test 2, 1.61.

## Discussion

In general, investigators have not considered the complex nature of space orientation in their experimental design. Too much attention has been paid to isolating the cues involved, and the spatial situations have often been left undefined. Contradictions concerning the role of the various sense organs may be due to the variety of experimental conditions employed. For example, Sobol (7), experimenting with carrier pigeons before and after bilateral electrocautery of the semicircular canals for short distances, found that they oriented themselves by means of vision, the vestibular apparatus being a secondary factor, if present at all. His results show that possibly the first moments of orientation, especially in long distance flights, are accomplished by means of the vestibular apparatus. Thus distance becomes a factor in determining which of the sense organs are involved. Finding oneself in an unfamiliar situation may elicit psychological processes far different from the ones involved when in a familiar situation.

In our experiments, the vestibular organs proved to be a detriment in one situation, and in another situation they were of little value. The fact that *S*s with vestibular sensitivity did significantly poorer on Test 1 may be due to the disturbance in orientation by the stimulation of

the semicircular canals when moving *S* over a short angular path.

This hypothesis seems to be confirmed by the results on Test 2. Here *S* was led in one straight path. The canals were not stimulated and, as we should expect, no significant differences occurred in the ability of the two groups of deaf *Ss* in returning to the initial position. On the other hand, it is possible that the vestibular-insensitive *Ss* compensated for their loss by developing and adjusting more readily to kinesthetic cues. The phenomenon of compensation has to be considered in the investigation on the function of sense organs in psychological processes. In an excellent review of the field of labyrinthine destruction and compensation and as a result of his own experiments on dogs, Asratian (1) concluded that compensation of the labyrinthine disturbances has a slow and markedly gradual character. The most superficial observation leaves no doubt that the mechanism of "learning" plays a significant role in this complex process. Thus, the ability of our deaf *Ss* in Group IV to return to the initial position in the orientation tests may be due to the fact that these tests relied heavily upon kinesthetic cues. Liebig (6) in his orientation studies without the use of vision demonstrated the importance of kinesthesis. He either led *S* or conveyed him in a small wagon to a different part of the room and *S* had to point toward the spot previously shown him. Orientation was more accurate when *S* was led and also when the path traversed was a straight line rather than curved. We may conclude, however, that under the conditions of our experiment the vestibular organs are not necessary for space orientation. It is possible that if our experiment had involved longer distances, the results may have been different.

Evidence for either hypothesis, disturbance in orientation by vestibular stimulation or compensatory functioning of kinesthetic organs, is secured from a comparison of the results of the present study with those of a similar study on blind and sighted *Ss* by Worchel (9). The results appear confusing in that neither hypothesis seems to be supported. If *Ss* who possess vestibular organs are disturbed on Test 1, then the "normal" *S* should do poorer on this test than the deaf in Group IV. This is not true as we noted in the results. Also the deaf in Group I should do as well as the normal since the main difference between these two groups lies in the hearing and both possess normal vestibular functioning, but the normals were significantly superior. It is unreasonable to suppose that hearing should be so important in the present situation although the hypothesis can be tested by plugging the ears of our "normal" *Ss*. On the other hand the results don't support the kinesthetic hypothesis either. If the deaf *Ss* of Group IV compensate for lack of vestibular functioning, they should do better than the "normal" *Ss*. This is not true, as we have seen. On the basis of these data, the introspections of our deaf *Ss* and the results of the adventitious blind on the orientation tests, the following hypothesis is proposed. Kinesthesis plus visual imagery are important in directional orientation involving short distances. The relative importance of the two processes depends on the distances involved. The longer the distances, the more important becomes the ability to visualize spatial relationships.

The education of deaf children emphasizes concrete imagery. A deaf child is taught to associate a verbal symbol with a concrete object. The ability to abstract relationships is retarded. Visualization of spatial movements is probably not developed to a high degree in the deaf. Therefore the inferior performance of the deaf children in Group I is probably due to their inability to develop a visual image of the spatial relationships involved. The better performance of the deaf *Ss* in Group IV may be due to the compensatory development of kinesthetic functioning. They use to the best advantage whatever sense organs they have left. This does not mean that kinesthesis is more sensitive but they utilize the kinesthetic cues more efficiently. The "normal" *Ss* develop a picture of the triangle as they are moved about the angular path and thus can perform as adequately as the deaf in Group IV. It is predicted that over long distances, deafened "normal" *Ss* would perform better on Test 1 than either of the deaf groups because of their ability to develop an image of the spatial relationships. Investigations are in progress to test this prediction.

Although the introspections of the deaf were vague, most of the reports mentioned "I just felt the way to go," or "I just figured it out." A few mentioned "counting steps." None of the *Ss* gave any indication of the use of visual imagery. The reports of the "normal" *Ss*, however, did show the use of a vague picture of the location of the initial position. From the earlier study (9), the "normal" *Ss* indicated that when they started out from the initial position along the first leg of the triangle, the direction of walking was important. As soon as they "turned" off this path to the second leg, they then "tried to keep in mind just where the starting position was located."

Also the difference in the means between the blind and the deaf (Group I) is not significant at the 5% level on Tests 1 and 2. As we found in the earlier study (9), neither the congenitally nor the adventitious blind reported visual imagery and they were significantly inferior to the "normal" group.

## Summary and Conclusions

In order to determine the role of the vestibular organs in space orientation, eight groups of four *Ss* each from the State School for the Deaf were selected on the basis of audiometric, standing, and rotation tests for a 2 × 2 × 2 factorial design: total vs. partial deafness, congenital vs. adventitious deafness, and vestibular sensitivity vs. vestibular insensitivity. The space orientation tests consisted of two parts: in Test 1, *S* is led around two legs of a triangular path, and he returns alone to the starting position, and in Test 2, he is led along the base of a triangular path, and he returns by way of the two legs to the initial position. The results shows that:

1. On Test 1, the deaf *Ss* without vestibular sensitivity did significantly better than those who were vestibular-sensitive.

2. On Test 2, there was no significant difference between the means of the vestibular-sensitive and vestibular-insensitive groups.

3. Degree of deafness, etiology (congenital vs. adventitious), and interactions between the variables were not significant factors in the orientation tests.

4. Stimulation of the semicircular canals and not the utricular-saccular organs was probably responsible for the inferior performance of the deaf *Ss* on Test 1.

5. Compensatory functioning of the kinesthetic organs is proposed as an explanation of the superior performance of the vestibular-deficient group on Test 1.

6. Comparisons of the results of the present study with those of a previous study by Worchel on blind and sighted *Ss* indicate the importance of kinesthesis and visual imagery in space orientation tasks.

7. There is no significant correlation between chronological age and space orientation.

8. There are no significant differences between the male and female *Ss* on the space orientation test.

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# THE PERCEIVED SLANT OF VISUAL SURFACES—OPTICAL AND GEOGRAPHICAL [1]

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One of the properties of a visual surface along with hardness, distance, and color-with-illumination, is that of slant. This term must be understood to include not-slanted as well as slanted; in other words the variable consists of opposite qualities having zero slant as a norm. There is evidence that *optical* slant, so-called, is determined by stimulation. When vision is monocular and the head is motionless, this quality seems to depend on the gradient of the density of the "texture" of the retinal image (1). The experiment which appeared to demonstrate this psychophysical correspondence, however, is defective in that the procedure failed to isolate the quality of optical slant from a congruent quality of *geographical* slant which accompanied it. This failure should be amended if possible. Moreover the two kinds of slant need to be defined and their relevance to space-perception discussed.

Consider first the impression of slant embodied in the face of an object—a bounded surface, or a segment of an array of surfaces. It can be studied in the following situation. The *O* sits in an ordinary room with his gaze horizontally straight ahead and fixates the center of a surface such as a sheet of textured cardboard. This surface is then rotated by *E* around a horizontal axis, either forward or backward. The quality of slant will increase as rotation increases, either ceilingwise or floorwise, until just after reaching the greatest possible slant the surface suddenly becomes an edge (3). Putting aside the question of change in shape, this situation provides variation in slant without variation in distance or any of the other qualities of a surface. It also shows that the quality of slant has an upper absolute threshold at the point where the surface becomes parallel to the line of sight. It should be noted, however, that in this experiment the inclination of the surface to the line of sight is so arranged as to have the same value as its inclination to the physical horizontal.

Consider next the impressions of slant embodied in a continuous plane surface filling most of the visual field. Take as an example the visual experience of a man standing on a level desert plain and looking about. This example is particularly significant since it is a kind of minimum perception for any sort of spatial behavior.[2] What he sees is a level ground extending to the horizon with himself standing on it. No impression of slant seems to be evident. But this perception of the earth is almost certainly a product of the integration of successive eye-fixations (2, ch. 8). Ordinarily the man is unaware of his saccadic eye-movements, but if he attempts to in-

[1] The experimental study here reported was carried out by Janet Crum Cornsweet under the general supervision of the senior author. The experiment was planned with the assistance of Howard Gruber. The research is part of a project carried out under Contract AF 41(128)-42 between Cornell University and the USAF School of Aviation Medicine.

[2] The man must confine his gaze to the ground instead of looking upward into the sky if he is to have the kind of space perception with which we are concerned. He must see a surface. This earthbound experience makes a better starting point for psychological theory than the ethereal void of classical space-perception.

trospect, he may discover that every fixation yields a clear momentary impression of a small segment of the ground which *does* have a kind of slant. As he looks downward toward his feet the slant approaches zero, as he looks upward the slant increases, as the center of clear vision approaches the horizon the slant becomes maximal, and at the horizon itself the land ceases to be a surface and becomes an edge.

The varying impressions of slant in this case are somewhat analogous to those obtained with the rotating segment of surface but there are several differences. In this situation increasing slant is accompanied by the impression of increasing distance. In this situation the momentary impressions of slant quickly add up to the experience of a single surface perpendicular to gravity, whereas in the former situation they integrate to the experience of a rotating object. In this situation the total perception is a product not only of successive retinal images but almost certainly of correlated postural-gravitational stimuli as well. Finally, in this situation the optical slant of the surface at the point of regard is not congruent with the geographical slant of the surface in the visual world. The two kinds of slant must obviously be distinguished. Optical slant seems to be a more abstract variable of experience, whose importance probably consists in being determined by fewer variables of stimulation.

The psychophysical experiment on the stimulus for optical slant already referred to (1) utilized in principle the rotating object situation with the line of regard horizontal and straight ahead. The gradient of texture-density of a single retinal image was systematically varied by increasing the density in either an upward or a downward direction and the quality of slant was found to increase in either a floorwise or a ceilingwise sense. The edges and therefore the shape of the surface were eliminated by presenting it behind a circular window in an upright screen, and accommodation for the surface was held constant by substituting for it a projected image on another translucent screen. The objection can be made that *O*s were really judging geographical slant, not optical slant. It is important, therefore, to perform an experiment in which optical slant must be perceived separately and in which the theoretical distinction between the two is verified by two different sets of discriminative judgments.

The ideal test for the theory of two kinds of slant would be to eliminate postural-gravitational stimulation entirely and determine whether the impression of optical slant remained. In the absence of a laboratory outside the earth's gravitational field, an adequate apparatus would be a spherical room falling freely in an elevator shaft, but this also presents difficulties. All the experimenter can do, therefore, is to arrange matters to produce an *incongruency* between the reference-axes of the eye itself and the reference-axes of the experimental room. He can then discover whether (*a*) the two kinds of slant are separately perceived and whether (*b*) the retinal density gradient will determine the impression of optical slant but not the impression of geographical slant.

Slant may be said to vary along one dimension from floorwise to ceilingwise and along another dimension from right-wallward to left-wallward. Although these terms are geographical, they may also be used to describe the dimensions of optical slant, which has reference only to the up-down and right-left directions of the retinal anatomy. In this terminology, slant must not be confused with what the writers

prefer to call *tilt*. This is a third and different kind of phenomenal inclination—a clockwise or counterclockwise rotation of a surface with respect either to the up-and-down axis of the retina or to the gravitational vertical. Although this phenomenon is important and has been much investigated, it is outside the scope of the present paper.

## Method

The hypothesis is that an impression of optical slant will be in correspondence with the direction of increasing texture-density in the retinal image when an accompanying impression of discrepant geographical slant is not in correspondence with it. Each *S* sat squarely in the experimental room with his head turned 45° to the left and fixed in a headrest set at a 45° angle to the walls of the room. He faced a vertical gray cardboard screen in which a circular window 17.5 cm. in diameter had been cut at eye-height. The plane of this screen was parallel to the back wall of the room, and at 45° to the line of sight. The center of the window was 50 cm. distant from the eye used for observation, which yielded an elliptical field of view 20° in height and somewhat less in width. Vision was monocular.

Through the window, at a further distance of 73 cm., *S* saw a textured surface mounted on a panel which could be rotated around a vertical axis over a considerable range without the edges of the panel becoming visible. The surface could be set either perpendicular to the line of sight (but at 45° to the screen and to the far wall of the room) or parallel to the screen and room (but at 45° to the line of sight). The task of *S* was to judge when the surface reached one of these two normal positions as the panel was slowly rotated by *E*, using a modified method of limits.

In the former position, perpendicular to the line of sight, he was discriminating optical slant. In the latter position, perpendicular to the axis of the body and room, he was discriminating geographical slant. In order to set the variable surface at its optical norm, *S* had to detect and eliminate any gradient of texture-density in the retinal image, either from right to left or from left to right. In order to set the surface at its geographical norm there had to be a strong gradient of density in the retinal image. Whatever the stimulus complex for this impression may be, if he has it, it is not a zero gradient of density.

As a control, each *S* was also required to judge the position of zero slant when the window and the center of the variable surface were straight ahead of the eye and the setup was square with the room. The norms were then congruent instead of discrepant. In the latter condition the window made a 20° circular projection in the visual field whereas in the former condition it made an elliptical projection.

TABLE 1

Accuracy of Judgments of Slant in Degrees with Surfaces of Irregular and Regular Textures

| Condition | Irregular Texture ($N$ = 10) | | Regular Texture ($N$ = 10) | |
|---|---|---|---|---|
| | Mean *SD* | Mean CE | Mean *SD* | Mean CE |
| Optical norm | 5.8 | 3.9 R | 4.4 | 4.4 R |
| Geographical norm | 10.6 | 4.3 L | 3.6 | 2.1 L |
| Congruent norms | 6.1 | .01 R | 2.2 | 0.9 R |

Ten judgments were obtained from each *S* on the norm of optical slant, ten on the norm of geographical slant, and ten more on the normal position when the two were congruent. Half of each set were ascending and half descending judgments, beginning from five randomly selected starting points on either side of the norm. The *E* rotated the panel until told to stop by *S*, but then permitted the rotation to be reversed or advanced until *S* was satisfied. Ten college student *S*s were used, the conditions of the experiment being counterbalanced among them.

The texture of the variable surface in this experiment consisted of a mottled black-and-white bookbinder's paper, without regular pattern or alignment of the elements. This was the *irregular* texture. The entire experiment was then repeated with ten new *S*s using a wallpaper composed of a complex plaid pattern having wholly linear and rectangular elements. This was the *regular* texture.

## Results

The consistency and accuracy of the perceptions of slant are given by the standard deviations and constant errors of the judgments, the former being the better measure. As may be seen from Table 1, the mean *SD* of the judgments of ten subjects was about 6° for optical slant and about 10° for geographical slant. These values might be considered as rough absolute thresholds for the detection of the two kinds of slant in this situation. It is

evident in the first place that the two can be perceived independently. The constant errors do reflect a tendency for the optical norm to be somewhat deflected toward the geographical norm and vice versa, but since the two are 45° apart the conclusion is nevertheless safe. When the norms are congruent, it may be noted, the threshold for slant does not greatly decrease (it is significantly lower than the geographical threshold but not lower than the optical threshold) but the constant error vanishes.

These data refer to the irregular texture. For the regular texture the results are also given in Table 1. In the case of this surface all judgments tend to be somewhat more accurate. Both optical slant and geographical slant are here significantly more consistent when their norms are congruent than when they are discrepant. The superiority of the optical over the geographical perceptions fails to appear in this experiment and they are not significantly different. The constant errors, as before, suggest that the two norms mutually attract one another when they are discrepant but that nevertheless they can be clearly distinguished and that deviations from them can be separately discriminated.

## Conclusions

The experiment shows that the two kinds of slant with which we are concerned can be distinguished by an ordinary observer in the situation described. There seems to exist a purely visual impression of slant for the segment of surface fixated which is dependent on its geometrical relation to the eye, not on its relation to other parts of the world or to gravity. The hypothesis that this quality corresponds to the gradient of density of "texture" at the fovea becomes reasonably certain. It has been demonstrated that when the head and eye are turned to one side a zero gradient of density yields a zero optical slant but a 45° geographical slant. It was already known (and confirmed here) that when the eye is pointed straight ahead a zero gradient of density will yield a zero optical slant and a zero geographical slant. It is therefore shown that optical slant corresponds to the gradient of density but that geographical slant does not. The implication of the earlier experiment on slant-perception (1) is upheld.

To what complex of stimuli, then, *does* geographical slant correspond? The impressions of geographical slant obtained were definite rather than ambiguous and hence probably determined by stimulation. This experiment does not give the answer, but it suggests that the angular rotation of the head and eye relative to the body has something to do with the question. A reasonable hypothesis would be that the postural-kinesthetic stimulation which goes with the turning of the head and eye is correlated with the retinal stimulation which yields optical slant, and that the two jointly determine geographical slant.

The question is, once we accept the density-gradient formula for optical slant, why does the perception of the geographical slant of a surface remain constant when the line of regard intersects it at varying angles? Why does a wall have the same slant when one looks 45° to the left as when one looks straight ahead? Why does the earth appear level as we lift our eyes from our feet all the way out to the horizon? The question is not simple, but the key to the answer may prove to be this: when there is a *compensatory relation* between the angular rotation of the eye and the angular optical slant, the perception of geographical slant remains constant. This would mean

that when the eye rotates to the left and the density-gradient toward the right-hand side of the retina becomes steeper (and if the product of these two variables is invariant), the perception of objective slant will be constant. Conceivably, this is why we see a level terrain as such.

According to this theory the situation of a man who stands on a physically *sloping* terrain is a special case. If he looks up or down the slope he perceives *slant* in our terminology; if he looks athwart the slope he perceives *tilt*. In this case the geographical perception is subject to illusions. The ordinary correspondence between the posture of the eye and the density gradient (or tilt) of the image has here been altered; there is a discrepancy or conflict of cues. The situation is important to understand, but it should not be confused with the basic one in which the covariance of postural and retinal stimulation seems to yield a stable and upright visual world.

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# DOES MOTION PERSPECTIVE INDEPENDENTLY PRODUCE THE IMPRESSION OF A RECEDING SURFACE? [1]

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Motion parallax as a fact of optics has been known for a long time. When an observer moves, the apparent relative motion of two or more stationary objects in the visual field is an indicator or clue to their relative distance. The theory that tridimensional perception depends on the interpretation of bidimensional sensations specifies a list of such clues or cues for depth and distance, and motion parallax is an important item on this list. Helmholtz argued that during locomotion, for instance, "the apparent angular velocities of objects in the field of view will be inversely proportional to their real distances away; and consequently safe conclusions can be drawn as to the real distance of the body" (**5**, p. 295). The reason why these sensations of velocity are experienced as distance rather than as velocity is that the process of interpretation or inference has become so habitual as to become unconscious. We learn to see relative motion as relative distance during the course of growing up in the environment. However, this perception is never more than a "safe conclusion." As Helmholtz was careful to point out, the cues for depth are not compelling for the impression of depth; they function as such only when experience makes the assumption of a third dimension reasonable.

The writer has suggested that this theory is formulated only for objects in space, and that the *background* of objects—the ground or terrain—is left out of account (**3**, ch. 7). When the retinal image is considered as the projection of a continuous array of textured surfaces, it becomes possible to state the fact of motion perspective more generally, as deformation or skew in the retinal image, and to analyze the gradients of velocity and direction. A more parsimonious theory then becomes possible by assuming that the velocity gradient for the elements of a surface is a stimulus for the impression of distance. Phenomenal distance is conceived as the recession of a surface, or an array of surfaces, rather than as the optical third dimension.

The decrease in "crossed" velocity in the visual field with the increase in distance may be termed *motion-perspective*, by analogy with the increase in the density of contours with increasing distance (**3**, p. 137 ff.). Size-perspective, texture-perspective, and linear perspective are all special cases of the rule that density in the visual field goes with recession in the visual world. The perspective of crossed diplopia usually accompanies the perspective of crossed velocity, but it is absent in monocular vision.

The experimental study of motion-perspective (as distinguished from the motion parallax of isolated objects in empty space) requires that adequate stimulation be present for the impression of an extended surface, i.e., a differentiated retinal image (**4**). A difficulty is then faced: how can *E* isolate motion-perspective from density-perspective? If a gradient of velocity in the retinal image is set up with an *even* distribution of elements, this

[1] The experiment here reported was carried out by Walter Carel under the general supervision of the senior author. The study is part of a research project performed under Contract AF 41(128)-42 between Cornell University and the USAF School of Aviation Medicine.

constitutes a zero gradient of density tending to produce a frontal surface which conflicts with the expected receding surface. If a gradient of velocity is set up with a congruent gradient of density, then the former has not been isolated. The latter case holds for the motion-picture methods of presenting velocity-gradients tried out during the last war, notably in the so-called "Landing Judgment Test" (2, ch. 9).

The conclusion seems to be that motion-perspective cannot be *isolated* from density-perspective, properly speaking, but can only be put in opposition to it. In the face of this difficulty, it is nevertheless important to discover whether an "elastic deformation" of the retinal image will overcome an even distribution of elements to produce the impression of a receding surface.

## Method

It was decided that the source of stimulation should be a bank of scattered points of light in a dark field of view, without alignment in rows and having a uniform *over-all* density. When motionless, this arrangement should produce an impression of spots on a frontal plane. The lights, however, should be capable of moving across the field, appearing on one side and disappearing on the other, with a velocity decreasing upward from the bottom of the bank to the top. With respect to velocity, the view would be similar to that produced at night by looking out the window of an airplane at the lights of a city. The difference would be that the city lights would also have a gradient of increasing density and decreasing size. The question to be answered was whether *O* would see an array of spots in the frontal plane when the lights were motionless but a plane receding upward when they were moved. Along with the latter impression there might be a feeling that *O* himself was moving.

*Procedure.*—The device constructed to produce this effect was a 6-ft. black disk mounted upright with a bearing at its center around which it could be rotated. Only a sector at the bottom of the disk would be visible at any one time. Its surface was entirely covered with narrow lines of luminous paint radiating outward from the center. The lines were broken segments, however, rather than continuous radii and these line-segments 6–8 in. long were spaced about as far apart at the periphery as at the center. In front of the disk was a screen, opaque in all parts except for a triangular area, base down, in which thin horizontal slits were cut, parallel and about an inch apart. A sector of the luminous lines was thus permitted to shine through the horizontal slits, making a roughly triangular field of points. The random distribution of the radiating segments prevented any alignment of the points such as to suggest linear perspective. The luminous paint was activated by a "black light" lamp, but otherwise the room was in darkness.

Rotation of the disk produced velocities of the spots approximately proportional to their distance from the center, i.e., a gradient of velocities. The velocity of any spot was not, however, quite uniform along its horizontal slit, being greater at the beginning and end of the path. For the case of motion-perspective as observed through the window of a plane or train, the velocity of a point is greater at the *middle* of the path. The motion-perspective obtained by the device, therefore, was only approximately that of a level terrain projected on a picture-plane.

Each *O* was allowed some minutes for dark-adaptation and then the motionless bank of spots was presented to one eye, at approximately the distance of the theoretical station-point. The line of sight was horizontal and the field occupied 30°–40° of visual angle. A report of the perception was then obtained. The disk was next set in rotation and the experience was again described. One group of 7 *O*s was made up of the *E*s themselves and other experienced *O*s who understood the apparatus and knew the purpose of the experiment. A second group of 10 *O*s was obtained who were ignorant of both.

## Results

All *O*s saw the motionless bank of lights in a plane perpendicular to the eye. No experience of slant, recession, or increasing distance was reported. When the lights were in motion, the "sophisticated" *O*s could see the spots progressively more distant toward the top, in a plane which receded. There were even reports of subjective motion in the direction opposite that of the lights. These descriptions are worth little, however, since they might have been influenced by expectation or suggestion. Only the spontaneous reports of the "naive" *O*s can be relied on, and these were

quite different. Six of the ten saw a group of isolated spots, some of which were moving faster than others, all at the same distance from the eye. The spots appeared to be separate objects, moving independently. Three of the ten reported something like the light of a city at night, with a receding plane of distance. One was wholly equivocal.

## Conclusions

The results of the experiment were negative. No compelling or universally obtained impression of recession, slant, or relative distance was induced by the skewed motion of the bank of lights, and even less evident was any impression that the *O* himself moved.

It was recognized at the outset that a bank of luminous points might not produce adequate stimulation for a visual surface. Small spots separated by large areas of darkness are quite different from the adjacent highlights and shadows of a texture. The bank of lights did not seem to have much "hardness" or "impenetrability"; there was an impression that one could see through them (4).

The majority of *O*s saw the lights as a collection of discrete objects in empty space, not as elements linked together in a surface. It seems likely that one property of a surface is phenomenal rigidity during all kinds of motion, and this property was weak or absent in the banks of lights.

The conclusion must be that under the conditions of this experiment the moving spots functioned truly as clues or cues for distance rather than as stimuli. As Helmholtz himself recognized, the cues for depth are not compelling or determining since attitudes, assumptions, or inferences must enter into the perceptual process. Motion parallax, then, can be perceived *either* as differences in velocity without differences in depth or differences in depth without differences in velocity. When the elements of the retinal image are separated points of light, and when the gradient of their motion is incompatible with the gradient of their density, the resulting perceptions of depth are ambiguous.

The fact that it is not possible to *isolate* a gradient of motion from a gradient of density but only to produce one which is incongruent with the other illustrates once more the primary status of the density variable in the retinal image for spatial impressions. Only when the image is composed of sharply differentiated regions (4), can the perspective gradients exist. The three main space-determining gradients (density, motion, and binocular disparity) might conceivably be related in this way: that only when they are congruent are they psychophysically determining; when incongruent, they serve merely as clues for probable inferences (1).

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# ACQUISITION AND RETENTION OF THREE PSYCHOMOTOR TESTS AS A FUNCTION OF DISTRIBUTION OF PRACTICE DURING ACQUISITION [1]

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The three studies to be described have the same general design and are intended to reveal information bearing upon the same problems. They are concerned with determining the nature of between-group differences during acquisition and retention produced by variation in spacing of practice during acquisition. Both paced and self-paced tasks were used since there is a suggestion in the literature (**5, 8**) that distribution affects these two types of tasks differentially.

Amount of practice was equated in two studies in terms of the response to be learned and in the third by the use of a constant-duration work period. The distribution of practice study is an exceedingly difficult one to design. If practice is equated in terms of length of practice period, variation in distribution makes for variation in number of correct responses, i.e., scoring responses, and number of erroneous responses. A continuous practice group can be expected to make fewer correct matches on the Complex Coordination Test or to be on target a lesser percentage of time on the Rotary Pursuit Test than a widely distributed practice group. Differences between groups during and, in particular, following practice can be ascribed to differences in opportunity to practice the correct responses. In some tasks, this is probably not too important as a source of variability. For example, in the Rudder Control Test, *S* apparently learns to *control* the cockpit rather than to move it in a certain way or hold it in a certain position. This may be learned as well or almost as well when *S* is on the target a lesser percentage of the time as when he is on target a greater percentage of the time.

If practice is equated in terms of responses to be learned, then *time* devoted to practice cannot be equated since more massed groups require more time to make the same responses or to achieve a learning criterion than do more distributed groups. The difficulty is not too great when the task involves a series of relatively discrete responses, with latency as the performance measure, and where the groups differ only in latency. Where *S* is doing something else when he is not making the correct responses, as is true with rotary pursuit, serial verbal learning, mirror tracing, learning of multiple unit mazes, and similar tasks, the problem of equating in terms of responses becomes more serious.

Equating practice in terms of responses seems more consistent with current theoretical formulations where number of responses or number of reinforced responses are the experimental units which enter into the theoretical formulations, than equating in terms of time, and for that reason has been used in two of the three studies in this report. Unfor-

[1] The experimental work for this study was performed as part of the United States Air Force Human Resources Research and Development Program. The opinions or conclusions contained in this report are those of the authors. They are not to be construed as necessarily reflecting the views or endorsement of the Department of the Air Force.

tunately, in many motor-learning situations, equating in terms of a constant-length practice period has great advantages as far as administering is concerned.

## Procedure

*Subjects.*—The *S*s were basic trainee airmen at Lackland Air Force Base. Within each study they were assigned randomly to the experimental groups. It will be noted that the *N*s for the retention data are at variance with those for the acquisition data. This nonavailability of *S*s for the later retention trials is due to the reconstitution of flights. The *S*s were identified by flight and name. When a flight was disbanded it was impossible to locate a subject since the name alone was not sufficient identification.

Recomputation of the acquisition data employing only those *S*s available for retention did not alter the results.

### *Rudder Control Experiment*

*Apparatus.*—The apparatus was an adaptation of the standard Rudder Control Test (CM120C) which has been described elsewhere (4). This test consists of a small cockpit mounted in unstable equilibrium on a shaft. The movement of the cockpit is controlled by two rudder pedals. A sight is attached to the front of the cockpit and *S* directs this towards one of the three target lights on a panel immediately in front of the apparatus. Score is in terms of time on target. In the present study, only the center light was used as a target.

At the beginning of each trial the center target light came on for 30 sec. The *S* was required to hold the nose of the cockpit in line with this light during the 30-sec. period. At the end of the 30-sec. period, one or the other of the end lights came on and the center light went off. This terminated the trial as far as the score was concerned, although *S* was not informed about this. The *S* attempted to align the sighting bar with the end light as quickly as possible. As soon as this was done, the end light was turned off. The inertia of the turning movement carried the cockpit past the end light position and into contact with a short metal post which checked the sidewise movement of the apparatus. To initiate a turning movement from this position, it was necessary for *S* to push hard on the opposite rudder and shift the weight of his body in the direction of the side on which the cockpit was positioned. The *S* could remain at rest when the cockpit was positioned at either side without exerting pressure on the rudder pedals or shifting the weight of his body.

As has been stated above, the rate of movement of the cockpit to either end position did not affect the score obtained by *S*. The use of the end lights was designed only to get *S* to an end position as quickly as possible, thus terminating a trial. The lights appeared in a regularly alternating sequence with the right light following the first trial, the left the second, etc.

*Training.*—A massed group (M) of 101 *S*s was given 12 30-sec. trials. When these *S*s moved to an end position, they were required to turn immediately towards the center target light. A distributed group (D) of 103 *S*s was treated in the same manner as Group M except that a 30-sec. rest period was given between trials.

*Retention.*—Eighty-four *S*s in each group were tested for retention ten weeks following the first training period. Twenty 15-sec. trials were administered without rest periods.

### *Complex Coordination Experiment*

*Apparatus.*—The Complex Coordination Test (CM701E), sometimes called the Mashburn Test, was used in this experiment. The test as described by Melton (4) was modified in two ways. First, the 13 red-light settings used in the standard form of the test were rearranged in a random sequence of 40 settings. Second, provision was made for turning off the display of red and green lights at the end of each block of ten settings. These blocks of ten settings will be referred to as trials. The score was in terms of time required for each trial.

*Training.*—Four groups of about 50 *S*s each (Total $N = 215$) were given eight trials. For one group these were given without rest intervals between trials. The remaining groups were differentiated on the basis of intervals of 12, 60, or 120 sec. between trials.

*Testing.*—Ten minutes following training a test trial of 10 settings was presented. This was followed by a second test trial 5 min. later.

*Retention.*—Ten weeks following training all available *S*s, about 45 in each of the four groups, (Total $N = 183$), were retested with 60 settings, presented continuously.

### *Rotary Pursuit Experiment*

*Apparatus.*—The rotary pursuit apparatus used in this experiment has been described by Melton (4).

*Training.*—Four groups of about 45 *S*s each (Total $N = 193$) were given 20 trials. The intervals between trials were 0, 12, 60, and 120 sec. for the various groups. A trial was defined as 6 sec. on the target. Timing was shifted from

TABLE 1

MEAN TIME-ON-TARGET SCORES AND *SD*'s (IN .001 MIN.) FOR MASSED (M) AND DISTRIBUTED (D) GROUPS ON THE RUDDER CONTROL TEST DURING ACQUISITION AND RETENTION*

| Acquisition | | | | | Retention | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Group Trials | D ($N$ = 103) | | M ($N$ = 101) | | Trials | D ($N$ = 84) | | M ($N$ = 84) | |
| | *M* | *SD* | *M* | *SD* | | *M* | *SD* | *M* | *SD* |
| 1 | 128 | 94 | 112 | 84 | 1 & 2 | 333 | 42 | 315 | 48 |
| 2 | 234 | 129 | 180 | 123 | 3 & 4 | 349 | 46 | 353 | 48 |
| 3 | 291 | 120 | 231 | 117 | 5 & 6 | 370 | 34 | 369 | 34 |
| 4 | 340 | 108 | 266 | 111 | 7 & 8 | 382 | 30 | 383 | 26 |
| 5 | 365 | 101 | 314 | 116 | 9 & 10 | 384 | 37 | 383 | 38 |
| 6 | 399 | 81 | 328 | 100 | 11 & 12 | 391 | 28 | 387 | 28 |
| 7 | 413 | 78 | 333 | 108 | 13 & 14 | 389 | 33 | 389 | 25 |
| 8 | 427 | 67 | 353 | 94 | 15 & 16 | 391 | 32 | 392 | 32 |
| 9 | 428 | 62 | 367 | 93 | 17 & 18 | 392 | 36 | 394 | 22 |
| 10 | 442 | 56 | 370 | 89 | 19 & 20 | 389 | 38 | 385 | 44 |
| 11 | 438 | 44 | 379 | 76 | | | | | |
| 12 | 452 | 41 | 377 | 84 | | | | | |

* Trials are 30 sec. (.500 min.) for acquisition and 15 sec. (.250 min.) for retention. For ease of comparison two-trial combinations are given for retention.

one clock to another at the end of each 6-sec. period and the click of a relay which effected this change indicated the completion of a 6-sec. trial to *S*.

*Testing.*—Ten minutes following training one 12-sec. on-target trial was presented. This was followed 5 min. later by a second 12-sec. on-target trial. These trials have been designated Test 1 and Test 2.

*Retention.*—Ten weeks following the completion of training, all groups were given 20 trials (6 sec. on target) with 24 sec. between trials. There were about 40 *Ss* from each of the four training groups (Total $N$ = 173).

## RESULTS

### *Rudder Control Experiment*

Mean trial scores during the acquisition and retention are presented in Table 1. For every trial after the first (where $t$ = 1.2), the $t$'s for the differences between the group means show that there is a difference significant beyond the 1% level of confidence between group means in favor of Group D. These differences between trial means are relatively constant for all trials from the second to the twelfth.

On the first retention trial there is a difference, significant at the 1% level of confidence, in favor of Group D, the mean for Group D being 174, and for Group M being 155. For all remaining trials of the retention series the groups perform at the same level.

### *Complex Coordination Experiment*

The results obtained in this experiment are in the same general direction as those obtained with the Rudder Control Test. Results for the various distribution groups for the final training trial, the two test trials, and the first and last retention trials are presented in Fig. 1. After the first acquisition trial, for which $F$ = .676, with 3 and 211 *df*, the differences between the groups are relatively constant from trial to trial. For example, the differences between means (in .001 min.) for the 0- and 120-sec. rest groups on Trials 1–8 taken in order are: .015; .324; .310; .351; .379; .356; .302; .354. On Trial 8 the differences between the groups, with the exception of the 0- vs. 12-sec. and the 60- vs. 120-sec. groups,

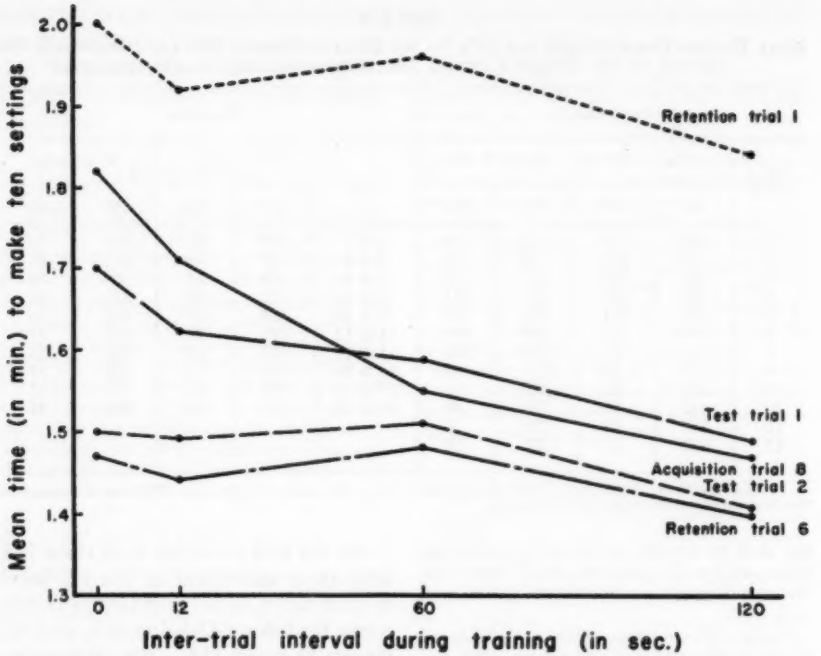


FIG. 1. Mean trial times (in minutes) to make ten settings on the Complex Coordination Test for acquisition Trial 8, Tests 1 and 2, and retention Trials 1 and 6

are significant at or beyond the 5% level of confidence.

On Test 1, which was given 10 min. following training, differences between groups are reduced. The difference between the 0- and 120-sec. groups on Test 1 is .205 min. On Test 2, which was given 5 min. later, the differences are reduced further, the 12-sec. group performing more proficiently than the 60-sec. group.

The means for retention Trials 1 and 6 are presented in Fig. 1. The 120-sec. group is superior to all other groups on the first retention trial. The $t$ for the difference between the 0- and 120-sec. groups at the first retention trial is significant at the 5% level of confidence. At the second and following retention trials, however, there is no significant difference between these two groups.

The results from analyses of variance for the last training trial, the first retention trial, and the last retention trial indicate that the groups differ significantly between themselves at the end of training but at no subsequent period. The analysis of variance for the last training trial must be interpreted with due caution, however, since the group variances are varying significantly among themselves.

As may be seen from Fig. 1, performance at the end of training tends to be a negative decay function of rest interval. The slope of the curve of this function decreases during Test 1 and approaches zero on Test 2 and on the retention trials. The single diver-

gence from this trend occurs in the case of the 120-sec. group which is consistently, if not reliably, better than the other groups.

*Rotary Pursuit Experiment*

The results of this experiment are consistent with those of the Complex Coordination study. The scores on the acquisition trials reveal a tendency for the differences between the groups to be constant from trial to trial. An analysis of variance of the first acquisition trial showed no significant differences between the groups ($F = 2.4$, $df = 3$ and 189). The differences between the 0- and 12-sec. groups tend to be constant as do the differences between the 60- and 120-sec. groups. The differences between the 0-sec. group on the one hand and both the 60- and 120-sec. groups on the other hand tend to decrease in the later acquisition trials, as do the differences between the 12-sec. group and the 60- and 120-sec. groups.

A conventional analysis of variance was not applied to the final trial data from the acquisition series since the group variances are differing significantly between themselves. An analysis of variance was carried out using the variance of the most variable group as the error estimate. This procedure has the disadvantage of reducing the degrees of freedom but the test would seem to be a conservative one. The $F$ ratio for between- over within-group variances is significant beyond the 1% level of confidence. With the exception of the 0- vs. 12-sec. and the 60- vs. 120-sec. groups, all other group

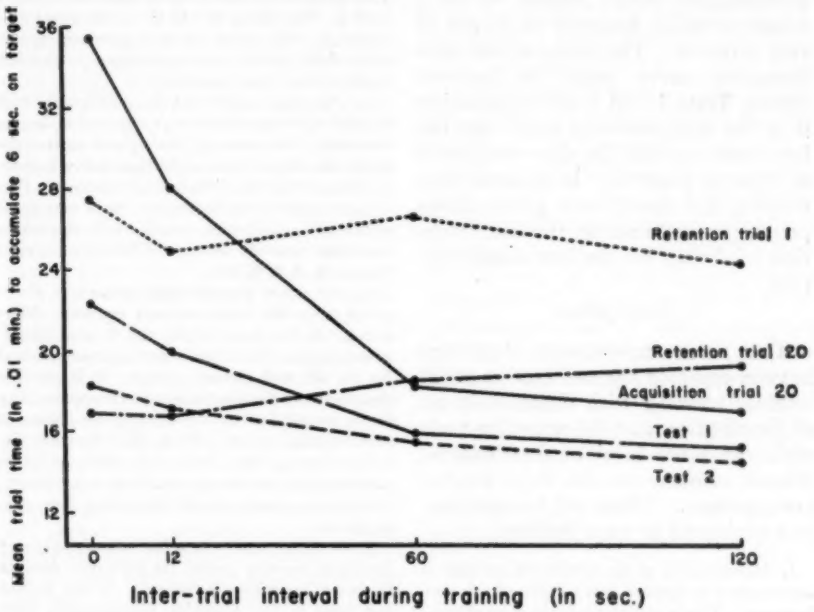


FIG. 2. Mean trial times (in .01 min.) for accumulating 6 sec. on target on Rotary Pursuit Test for acquisition Trial 20, Tests 1 and 2, and retention Trials 1 and 20. The on-target period of 12 sec. for Test 1 and Test 2 has been halved.

differences are significant at the 1% level of confidence or beyond, on the twentieth acquisition trial.

Differences between groups are reduced during Test 1 and Test 2. The group variances are differing significantly at Test 1 but not at Test 2, although apparent differences are present at Test 2. The *F* ratio based upon between- and within-group differences during Test 2 is significant beyond the 1% level of confidence.

Analyses of variance of the retention trials show that differences between groups at the first and last retention trials are not significant. Also the differences between extreme groups as defined by the experimental variable are not significant.

Figure 2 shows performance as a function of distribution at selected trials. At the final acquisition trial, performance would appear to be a negative decay function of length of rest interval. The slope of the performance curve tends to decrease during Tests 1 and 2 and approaches 0 at the first retention trial. At the last retention trial the slope is either 0 or slightly positive. It is interesting to note that the 120-sec. group shows poorer performance on the last retention trial than on the last acquisition trial.

## Discussion

The three experiments described have in common the two factors of (*a*) original training with different levels of distribution and (*b*) retention tests with the same level of distribution. Trends common to the three studies are apparent. These will be described and evaluated in what follows.

1. *Manipulation of the distribution variable is accompanied by between group differences in performance.* This is not surprising in view of commonly observed results of distribution of practice studies, although some studies have revealed no difference between groups trained under different levels of distribution (3). Among these, studies of distribution with the Complex Coordination Test (5, 8) revealed little if any difference between groups as a function of distribution. The most obvious difference between these studies and the present one with the same apparatus is that the Form *E* of the Complex Coordination Test used in the present study is a more difficult form of the test than that employed in the earlier studies.

2. *Differences between groups are established relatively early in training.* It will be noted that clear differences between groups are established at the second trial with the rotary pursuit and rudder control tasks. With the Complex Coordination Test, the groups are differentiated at the second trial in a manner consistent with the differentiation of the variable. Clear differentiation of all groups is apparent at the fourth trials. The 0- and 120-sec. groups are as well differentiated at Trial 2 as at the final trial. It would seem that distribution is not only an important determinant of performance but that its effects are noticeable immediately.

3. *Performance tends to be a negative decay function of the interval between trials.* This has been pointed out earlier and is apparent in Fig. 1 and 2. One thing should be noted about this function. The curve which it generates apparently falls rapidly and approaches its limiting value within a few minutes.

4. *After the earlier trials in a series, the trial to trial differences between groups tend to become constant.* That is to say, throughout most of the trials the slope of the acquisition curve is independent of the conditions of distribution. Previously reported results are consistent with those obtained in the present studies with respect to the trend towards constant differences between groups (4, 5, 6, 9, 10).

In the rotary pursuit study there is a divergence from the trend towards constant differences. In the later trials, the 0- and 12-sec. groups approach the level of performance attained by the 60- and 120-sec. groups. It is possible that the more distributed groups are approaching limits established by the existing conditions of presentation of trials. Thus, while the more distributed groups have reached a ceiling as far as performance under these conditions is concerned, the massed groups are still improving their performance.

5. *In the test trials run a few minutes following the initial training period, the differences between the groups are markedly reduced.* At the second test trial in the Complex Coordination Test study, for example, there is little real difference between the 0-, 12- and 60-sec. groups. At the second test trial with rotary pursuit, it is inter-

esting to note that the 0-sec. group performs at approximately the same level as the 60-sec. group at the last acquisition trial.

6. *At the first retention trial the groups tend to be differentiated in the same manner as at the end of training.* The main divergence from the trend occurs in the case of the 60-sec. groups in the complex coordination and rotary pursuit studies, where performance falls below the level observed for the 12-sec. groups. As at the test immediately following training, the between-group differences at the first retention trial are considerably reduced over those holding at the end of acquisition.

7. *With continued testing during the retention series, between-group differences tend to dissipate.* There are two exceptions to the general trend. These merit some comment.

First, although between-group differences are not significant, in the case of the complex coordination study, the performance for the 120-sec. group is consistently better than that of the other groups. Second, in the rotary pursuit study, the two more massed groups perform more proficiently at the final retention trial than do the two more distributed groups.

The first noted result is the only evidence for any persistent residual effects of prior conditions of distribution on the three studies reported here.

The second exception to the general trend in the data, in the case of rotary pursuit, has at least two possible explanations. First, because of the method of equating practice, i.e., in terms of the response to be learned, the more massed groups spend a greater amount of time trying to follow the target than do the more distributed groups. Although the more massed groups are not actually following the target during all this time, they are doing something which can be expected to be related to their future behavior as this concerns rotary pursuit. The massed groups may be better simply because they have had more relevant practice in learning rotary pursuit. There is one obvious difficulty with this explanation. The 120-sec. group performs more proficiently than the 0- or 12-sec. groups during the first few retention trials. If the 0- and 12-sec. groups have had greater amounts of relevant practice, these groups could be expected to perform better from the beginning. They do not.

A second explanation is one suggested in an earlier paper (7). The detailed theoretical development required for a more adequate explanation will appear in a subsequent paper. Stated briefly, and in general terms, the difference favoring the more massed groups occurs because the conditions of trial presentation holding during retention are more similar to those holding during acquisition for the more massed groups than for the more distributed groups.

It is perhaps presumptive to attempt to state any firm generalizations about distribution of practice phenomena when so little is known about them. The results obtained in the present study and in previous ones are not, in the main, inconsistent with what follows. It seems not unlikely that any learning theory which is aimed at the explanation of distribution of practice phenomena will need to conform generally to the summarization outlined below.

With initiation of practice with a given rate of trial presentation, i.e., a given distribution of practice, there is a period occurring relatively early in training during which *S* adjusts to the conditions of practice. During this period between-group differences become established. Following the period of adjustment, *S* establishes a relatively steady state with respect to level of performing. Steady states are reached earlier with massed than with distributed trials. With the establishment of such steady states, differences between groups become stabilized. If the conditions of distribution are changed, there is a relatively brief period of readjustment followed by establishment of a new steady state the general level of which is a function of the conditions of trial presentation. Residual effects of previous conditions of distribution are quickly dissipated. Such residuals as persist arise because of generalization of the conditions of distribution holding at a moment from conditions previously established. This is probably a less important determinant than the conditions of distribution holding at a given moment.

## Summary

The present study was concerned with the determination of between-group differences during acquisition

and retention produced by variation in distribution of practice during acquisition. Three separate studies were done, using the Rudder Control Test (RC), the Complex Coordination Test (CC), and the Rotary Pursuit Test (RP). For the RC there were two groups, one massed and one distributed. For the CC and RP four separate groups were run, each with a different level of distribution. For all three studies the groups were tested on retention at a later time, under the same level of distribution in each study.

The results were as follows:

1. Manipulation of the distribution variable was accompanied by between-group differences during acquisition.
2. These between-group differences were established relatively early in acquisition.
3. Performance tended to be a negative decay function of the intertrial interval.
4. After the initial trials, the trial to trial differences tend to become constant.
5. Test trials run a few minutes after acquisition indicated a markedly reduced difference between distribution groups.
6. On the first retention trial the groups tended to be differentiated in the same manner as at the end of acquisition.
7. With continued practice during retention, between-group differences tended to disappear.

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# A CRITICISM OF PRE-ACQUISITION AND PRE-EXTINCTION OF EXPECTANCIES

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In 1946, Thorndike (3) took issue with Tolman's concept of expectancy and challenged interested parties to test the validity of this concept in four novel situations. He thought of these situations as crucial experiments in which rats would be given opportunities to acquire expectancies without developing any specific motor habits to go along with the expectancy. Control rats would learn the motor components from the start. The two groups would then be compared for efficiency of learning the total act. The situations, briefly described, were as follows:

1. Convey a rat through a simple maze in a little wire cart. Compare later learning with that of a rat who does not have such service.
2. Carry a rat by hand along the rungs of a little ladder to a platform at the top where food is available. Compare future learning to climb the ladder with the learning of animals without such prelearning experience.
3. Hold a rat gently and with your hand manipulate his front legs in such a way as to tear down a tissue paper barrier over the doorway of a food box. Compare as above.
4. Repeat Situation 3 in a situation involving digging away sawdust.

There has been no published comment on Thorndike's proposed crucial tests. The senior author has had students perform these demonstrations many times, and the results fully support Thorndike's predictions in that control rats uniformly do as well as or better than the experimental *Ss*. Thorndike may have been writing with tongue in cheek, or perhaps on the basis of unreported experimental evidence. In one test of Situation 3, an experimental rat remained in a test box for 48 hr. without tearing down the tissue paper barrier even though *E* punctured the paper in several places.

In discussing such demonstrations with supporters of expectancy theory, the objection is usually raised that Thorndike was unfair to expectancy views in that the chosen situations provided occasions for emotional disturbance or for the learning of expectancies that are not tested for in the demonstrations; thus, a rat might learn, in Situation 1, to expect to get a ride to the goal and is sitting around waiting for his conveyance. Since the rats do not sit around in this situation, but rather, travel about and explore just like control rats, such criticism is not well taken. It is also pointed out that even human passengers in buses, automobiles, etc. do not always learn a frequently traveled route and have to learn for themselves when they are in the driver's seat.

The difficulties involved in demonstrating an expectancy function in direct learning, if such exists, may well be too great to permit a crucial demonstration. In 1949, Seward and Levy (2), following through on some earlier investigations of Tolman, decided that the functioning of expectancies could be more readily observed in the extinction of some learned response, if *Ss* could first discover or otherwise learn that the previously rewarded response was now unlikely to lead to a reward. Accordingly, they trained rats to run a simple alley to a goal platform for food. After learning, one group was given *pre-*

*extinction* experience consisting of five 2-min. sessions on the empty goal platform. This procedure was calculated to inform the rats that it was useless to run down the elevated alley to get to the foodless goal. A control group had similarly timed sessions on a neutral platform. The results of the extinction trials showed a clear superiority for the experimental rats (see Table 1). These data were taken by Seward and Levy and later by Deese (1) to indicate that, as Deese remarks ". . . a simple non-choice response can be reduced in strength by 'telling' the animals that there is no longer food in the goal box" (1, p. 362).

Actually, the data indicate no such conclusion because of a weakness in the experimental design of Seward and Levy. During the crucial test trials, the experimental *Ss* with pre-extinction experience were allowed to stay on the empty goal platform for 120 sec. between trials. The control *Ss* were removed from the goal platform after only 20 sec. and spent the remaining 100 sec. on the neutral platform. Thus, there were two differences between the experimental and control groups instead of one: (*a*) the nature of pre-extinction experience, and (*b*) the amount of time spent on the goal platform between trials. In the absence of additional information about the effects of time on the platform in extinction trials, no conclusion can be drawn about the pre-extinction effects.

In the present study the Seward and Levy experiment was repeated with the required additional control groups consisting of rats that differed only in pre-extinction experience. Because the results were not in keeping with the Seward and Levy findings, another experiment was performed along the same lines, but with a different apparatus.

## Experiment I

*Apparatus.*—The apparatus was built to correspond in every detail with that of Seward and Levy. The only difference was in the use of white pine instead of redwood. In essence it consisted of an elevated strip with start and goal platforms.

*Procedure.*—The procedure also duplicated that described by Seward and Levy with the addition of a third group of *Ss* so that the experimental design involved: (*a*) *Experimental Group:* This group had pre-extinction training on goal platform and extinction trials with 120 sec. detention on the goal platform between trials. (*b*) *Control Group I* (like Seward and Levy): This group had pre-extinction training on a neutral platform and extinction trials with 20 sec. on the goal platform and 100 sec. on the neutral platform. (*c*) *Control Group II:* This group had pre-extinction on a neutral platform and extinction trials with 120 sec. on the goal platform between trials.

*Subjects.*—The *Ss* were 84 albino rats from the University of Buffalo colony. They were distributed as follows: Experimental group, 28; Control group I, 32, and Control group II, 24. The rats were 3–4 months old. Those used by Seward and Levy were 4½ to 5½ months old. In each of our groups, half of the *Ss* were male, half female.

*Results.*—The number of runs to the criterion used by Seward and Levy, two successive failures to leave the starting platform within 3 min., are shown in Table 1. The data of Seward and Levy are included for comparison. It is evident that there are no significant differences among the three groups in our experiment. It will be noted that all of our groups are larger than those of Seward and Levy and that the distribution of runs in both experiments is extensive. The variability among the *Ss* suggested that no meaningful measures of central tendency were likely to be found in small samples of *Ss* in this type of experiment.

The *Ss* in all three groups showed about the same types of running patterns and gave no behavioral indications of insight, expectancy, or frustrated expectancy. After the first few

TABLE 1

FREQUENCY DISTRIBUTION OF TRIALS TO REACH A CRITERION OF EXTINCTION

| No. of Trials | Seward and Levy | | Present Experiment | | |
|---|---|---|---|---|---|
| | Exp. | Control | Exp. | Control I | Control II |
| 0 | 1 | | | | |
| 1 | 4 | 2 | | | |
| 2 | 4 | 1 | 1 | 1 | |
| 3 | 3 | 1 | 1 | 2 | |
| 4 | 1 | | 1 | 6 | 2 |
| 5 | 2 | 1 | 4 | 3 | 3 |
| 6 | | 4 | 4 | | 1 |
| 7 | 1 | | 3 | 2 | 5 |
| 8 | | 2 | | 2 | 1 |
| 9 | | 1 | 1 | | |
| 10 | | 1 | 1 | 1 | 1 |
| 11 | 1 | | 1 | 1 | 2 |
| 12 | | 1 | 1 | | 1 |
| 13 | | | 4 | | 2 |
| 14 | | | 1 | 2 | 1 |
| 15 | | | | 3 | 2 |
| 16 | | | 1 | 1 | 1 |
| 17 | | | | | |
| 18 | | | | | 1 |
| 19 | | | | | |
| 20 | | | 1 | 1 | |
| 21 | | | | 2 | 1 |
| 22 | | | | 3 | |
| 23 | | 1 | 2 | 2 | |
| 24 | | | 1 | | |
| 25 | | 1 | | | |
| *N* | 17 | 16 | 28 | 32 | 24 |
| Mean | 3.12 | 8.25 | 10.28 | 11.28 | 10.04 |
| *SD* | 2.63 | 6.71 | 6.41 | 7.22 | 4.67 |

runs, *S*s gave up investigating the food cup and sat quietly on the goal platform or tried to escape from it. The runs continued in a more or less all-or-none fashion. The twentieth run might be as brisk as the first, even though on the twentieth run *S* made no attempt to approach the food cup. After the first few trials, *S*s in all three groups characteristically ignored the food cup. Since this was not an anticipated finding, we did not record the specific features of goal platform activity, but it should be emphasized that *S*s performed the running response, in many cases for a large number of trials, without bothering to look into the food cup which was attached just below the far edge of the goal platform. The purposelessness of these responses was striking.

## EXPERIMENT II

In considering the results just described, it was felt that the situation used was inadequate for showing an expectancy function because of the similarity of start and goal platforms (the only difference was in width). The Thorndike ladder situation, mentioned earlier, appeared to be more suitable in this respect and it was decided to repeat the experiment using the ladder problem.

*Procedure.*—Twenty rats, like those in Exp. I, served as *S*s. They were all trained to climb a small ladder to reach an enclosed platform mounted on a 2-ft. pole in the center of a 3-ft. sq. box. The apparatus was built according to Thorndike's directions. Following 20 massed training trials, two groups of *S*s were matched for running speed. After 24 hr., pre-extinction experience was given both groups, the experimental *S*s being confined on the goal platform just as in Exp. I. Control *S*s were confined on a distinctly different platform for similar periods. Regular extinction runs were then observed until each *S* failed to reach the goal platform in two successive 3-min. trials. All *S*s were confined for 2 min. on the goal platform on every trial prior to reaching the criterion.

*Results.*—The mean number of trials to reach the criterion for the experimental *S*s was 17.7; The *SD* was 15.88. For the control *S*s, the mean and *SD* were 12.3 and 11.27. While this difference favors the control *S*s, it is not significant ($t = .83$). Because the only issue is whether the experimental rats profited from the pre-extinction experience, and this appears to be clearly in the negative, no further consideration need be given these data. The small samples and extreme variability preclude confident conclusions from this experiment, but subsequent samples run by student *E*s have indicated that there is no reason to expect

that pre-extinction exposure to an empty platform conveys useful "information" to albino rats.

## Discussion

The results of both experiments provide no evidence that rats learn anything useful in a pre-extinction experience in a simple runway or a ladder problem. The results of Seward and Levy (2) are at first appearance equivocal in that they overlooked the presence of an extra variable whose role is not understood. Even when this variable is controlled, as in the present experiments, no differences are found between rats who are given a chance to acquire information and those without such opportunities. The rats used by Seward and Levy, in general, gave up running much sooner than did our own. This may have been due to an age factor; the younger rats in our experiment may have been more vigorous. It is possible, of course, that older rats benefit more from pre-extinction experiences of the type studied and this possibility might merit further exploration.

The continued interest in expectancy theory demands that conclusions in its favor be supported by clear-cut and unequivocal experimental results. The paper by Deese (1) describes an experiment patterned after that of Seward and Levy in a T-maze discrimination problem. In the Deese experiment, the design is also weak because control *S*s are not given any controlled pre-extinction. In the designs of Seward and Levy and Deese there is some question about the nature of the expectancy being tested. Does the rat learn that there is no food at the goal under *any* conditions or only if he is placed directly at the goal by *E*? If the latter is the case, then Seward and Levy apparently expect the rat to generalize from the situation where he is placed on the goal to that where he runs from a starting position. Since the majority of rats run more than zero trials, it is possible to explain later failures to run as due to the greater extinction of secondary reward values of the empty goal platform for the rats with pre-extinction experience (if they should happen to run less frequently than animals without such prior exposure to an empty goal box). Whenever a variety of plausible explanations is applicable to some phenomenon, the experimental design must be particularly precise.

## Summary

1. The proposals made by Thorndike for testing the acquisition of expectancies were implemented and found fruitless as predicted by Thorndike.

2. The experiment of Seward and Levy on extinction of expectancies is criticized because of a weakness of experimental design in that an uncontrolled variable was introduced in their procedure.

3. Repetition of the Seward and Levy experiment resulted in findings that do not support Seward and Levy even when the additional variable is not controlled.

4. Another experiment using a situation designed to be more likely to demonstrate extinction of expectancy also resulted in failure to show benefits from pre-extinction experience.

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# EXPERIMENTAL STUDIES IN ROTE-LEARNING THEORY: X. PRE-LEARNING SYLLABLE FAMILIARIZATION AND THE LENGTH-DIFFICULTY RELATIONSHIP [1]

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For the determination of precise quantitative relationships in learning, Ebbinghaus (2) long ago pointed out that meaningful materials have the limitation that they involve previous associations which affect the acquiring of new relationships. His solution to the difficulty was the invention of nonsense syllables, artificial combinations of letters without previous associations. But the virtue of novelty which these materials possess is also a limitation. In learning successive pairs of such syllables or in learning lists of them in serial order one must not only learn the associations between the units but must also "learn" the units to be associated. For certain types of theoretical analysis it would be desirable to analyze the effects of these two processes separately. In studying associational interferences, for example, one may be concerned with the number of repetitions required for learning as a function of the number of units involved in a serial chain, and consequently not wish to include the effects of initial familiarization with the units to be learned serially. Here, one would need units which are free from previously learned associations, and, at the same time, sufficiently familiar to permit the learning trials to reflect primarily the process of serialization. Meaningful words do not meet the first requisite, while ordinary nonsense syllables fail on the second. Spelling of individual letters and the use of numerals are methods that have been used in an attempt to satisfy the requirements, but previous associations between letters or numerals may present some of the same difficulties that are involved in meaningful words. Another method is employed in the present experiment: that of using nonsense syllables which are made "familiar" by methods which do not involve forming associations between them. The *Ss* studied the nonsense syllables presented in constantly changing order until they knew the syllables well enough to recognize them and to supply missing letters when parts of the syllables were presented individually. Following this process of familiarization the lists were learned in the conventional manner. Under control conditions lists were learned without prior familiarization. The effects of familiarization were investigated as a methodological factor affecting the relationship between length of list and number of trials required for learning.

[1] The writers wish to acknowledge with thanks the assistance of Dr. Samuel Brownell of the New Haven Teachers College in securing *Ss* and the helpful suggestions of Drs. Fred Sheffield and Gregory Kimble in the reading of the manuscript.

## Procedure

Three different lengths of list, consisting of 6, 12, or 24 syllables, were learned under both experimental (familiarization—Groups I, III, and V) and control (no-familiarization—Groups II, IV, and VI) conditions. Each of 12 *Ss* served once in each of the six conditions, with order of conditions counterbalanced between *Ss* for practice effects.

*Learning materials.*—The experiment required that the learning materials for different lengths of list and for experimental and control condi-

tions be as comparable as possible and that the familiarization procedure produce comparable familiarity with the units of lists of varying length. Six lists of 24 syllables each were constructed as parent-lists from which all smaller lists were derived. Each list of 24 could be considered as consisting of four sets of six syllables which could be used individually as 6-syllable lists. The first six syllables of each list were combined with the second set of six, and the third group of six with the fourth group to form two lists of 12 syllables from each of the basic lists of 24. For example, if the four lists of six syllables are designated as A, B, C, and D, the parent list of 24 would consist of ABCD and the two lists of 12 would be AB and CD. The six syllables within a segment were kept in the same order throughout.

Repetition of initial or final consonants within the lists was reduced to the practical minimum. With a single exception any given letter was not repeated within three consecutive syllables. Combinations of two letters were not repeated within any list in the same order. In applying this rule consonants grouped as phonetically similar were regarded as identical. Letters used in the first syllable of a list were not repeated in the last syllable (with one exception in a list of six). Any given syllable (or a phonetically identical one) was used only once in the entire set of lists.

The Glaze (4) association values of the individual syllables used ranged from 0% to 46.7%. The mean association value of all lengths of list was 22.7%. The range of mean association values was, for the 6-unit lists, 21.1% to 24.5%; for the 12-unit lists, 22.2% to 23.3%; and for the 24-unit lists, 22.5% to 22.8%.

*Familiarization.*—Familiarization was effected by presenting a set of syllables to *S* one at a time, changing the order for each run through the set. To maintain uniform familiarization independently of length of list to be learned, the entire parent set of 24 syllables was used in the familiarization preceding the learning of any of its segments.

A familiarization pack was constructed for each parent set by printing the syllables individually on 3- by 5-in. index cards. For each familiarization pack three test packs were constructed in a similar fashion, substituting a dash for one of the three letters of each syllable. Each letter of a given syllable was omitted in one of the three packs. Omissions in each letter position were equally represented in any given pack.

During familiarization, *E* and *S* sat facing each other at a table. The *E* held the familiarization pack facing *S* and exposed each successive syllable at a 2-sec. rate by allowing the preceding card to fall face down on the table, timing the presentation by the sweep-second movement of a stop watch. The *S* read each syllable aloud as it was exposed. A trial consisted of presenting all 24 syllables once each, and required a total of 48 sec.

Trials were presented at a rate not exceeding one every 2 min. To prevent the familiarization sessions from becoming inordinately tedious, *S* and *E* engaged in spontaneous conversation between trials. Consequently, a rigid rate of presentation of trials was not maintained, and the interval between trials was allowed to exceed the minimum of 72 sec. when a break in conversation at this point would be strained and unnatural.

After every set of five trials *S* was given one trial with a test pack, during which he attempted to reproduce (pronounce as a unit) each complete syllable when presented with only two of its letters. If *S* did not give the correct response within 3 sec., the syllable was scored as incorrect and *E* told *S* the correct response. Familiarization on a given pack was continued until *S* responded correctly on all syllables in a single test trial. Two or more sessions were usually required to attain this criterion.

Three sets of five familiarization trials with a test after every five trials were given on the first day for any pack and four such sets on each additional day required. When *S* reached the criterion on the first test run of any session, the appropriate serial learning for that pack was carried out in the same session. Otherwise learning was postponed to the following session and was preceded during that session by one set of five familiarization trials and a test. This provided that learning of any familiarized list was always immediately preceded by a set of familiarization trials.

To keep the length of the experiment within reasonable limits, the familiarization procedure was slightly modified if familiarization on any pack extended beyond two sessions. If *S* failed to respond to a test card within the 3-sec. interval he was not corrected by the *E* and that card was presented again at the end of the test trial. If an incorrect response was made to any card, the procedure of correction after 3 sec. was again instituted.

*Order of presentation of conditions.*—The order in which the 12 *S*s learned the lists under the six conditions was arranged to counterbalance practice from session to session. Thus, each condition appeared twice (once each for two different *S*s) in each practice position in a typical cross-over design. Randomization of order was further restricted by arranging the familiarized condition first in half the cases and second in the other half separately for both the 24- and 6-unit lists.

Each *S* reported over a series of daily sessions consisting of six serial-learning sessions and the

necessary familiarization sessions. Only one list was learned serially at any learning session. Familiarization sessions for a given list were interpolated between the learning session for that list and the preceding learning session. The total number of sessions varied depending upon the number of sessions required by various *Ss* to reach the familiarization criterion.

*Apparatus for serial learning.*—The syllables were presented by the Hull memory drum. The apparatus and *E* were situated behind a black screen 6 ft. high by 4 ft. wide with a 4- by 5-in. opening for displaying the syllable apertures.

Syllables were pronounced as a unit and learned serially by the anticipation method. The lists were prepared with a special typewriter on bands of semiglazed white paper in ¼-in. black uppercase letters and presented one at a time in fixed serial order at a 2-sec. rate. On each trial the first syllable was preceded by an asterisk presented for 2 sec. as a starting cue. Trials were massed, with the asterisk starting cue following immediately after the final syllable.

*Preliminary training.*—Prior to serving in the experimental conditions each *S* was given several days' preliminary training. The first day was spent learning two lists, one each of lengths 12 and 6, both without familiarization. Preliminary training was then completed by the learning of a 24-syllable list with the familiarization procedure. The same set of preliminary lists was used for all *Ss*.

*Instructions.*—Oral instructions were given in a uniform manner at several stages of the preliminary training as each novel task was encountered.

On the first day the rationale of the experiment was explained as a comparison between learning new and learning familiar syllables. Detailed instructions for serial learning similar to those previously used in these laboratories were given before learning the first list. Following this list *Ss* were reassured that making mistakes was common to all *Ss* and that failing to follow the first perfect trial immediately with another perfect recitation was customary. The *Ss* were encouraged not to be upset by such occurrences.

At the first familiarization session *S* was instructed to "*. . . pronounce each [syllable] out loud and study it so that you get to know it and recognize it when you see it.*" It was explained they were not to be learned in serial order and that the cards would be shuffled between each trial to prevent this. The test pack and procedure were then explained.

To keep initial *g* phonetically distinct from initial *j*, hard *g* was adopted as a standard pronunciation. Similarly, initial *q* was given a standard pronunciation of *qw* to keep it distinct from *c* and *k*. If these standard pronunciations were not spontaneously adopted by *S* for any particular syllable during familiarization, *E* suggested the standard to *S*.

*Subjects.*—A group of 12 men, consisting of 2 University of Connecticut undergraduates, 3 Yale undergraduates, and 7 Yale graduate and professional students served as paid *Ss*. The median age was 25 yr. with a range of 20 to 31 yr. Two additional *Ss* were rejected after preliminary training because of unusual difficulty with the experimental tasks.

TABLE 1

MEAN REPETITIONS REQUIRED TO LEARN VARYING LENGTHS OF LIST UNDER FAMILIARIZATION AND NO-FAMILIARIZATION CONDITIONS

| Length of List | To 1 Perfect Trial | | | To 2 Suc. Perfect Trials | | |
|---|---|---|---|---|---|---|
| | Famil. | No-Famil. | *p*<sub>*diff.*</sub> | Famil. | No-Famil. | *p*<sub>*diff.*</sub> |
| 6 | 2.2 | 4.1 | .002 | 3.6 | 5.9 | .0003 |
| 12 | 11.4 | 15.9 | .04 | 14.2 | 19.5 | .01 |
| 24 | 30.2 | 33.0 | .15 | 38.7 | 45.9 | .13 |

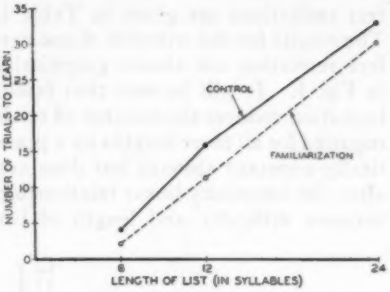


FIG. 1. The number of trials to learn to a criterion of one perfect recitation as a function of length of list under familiarization and control procedures

## RESULTS

The principal results are the comparison of the number of trials to learn the 6-, 12-, and 24-unit lists with and without prior familiarization. The mean number of trials to learn the three lengths of list to one perfect recitation and to two successive per-

fect recitations are given in Table 1. The results for the criterion of one perfect recitation are shown graphically in Fig. 1. It will be seen that familiarization reduces the number of trials required for all three lengths by a practically constant amount but does not alter the essentially linear relationship between difficulty and length of list found in this range of lengths with the ordinary nonsense syllable procedure both in the present experiment and in previous studies by Ebbinghaus (2), Meumann (7), and others. This indicates that complications arising from the necessity of becoming familiar with units in the ordinary nonsense syllable technique does not introduce serious

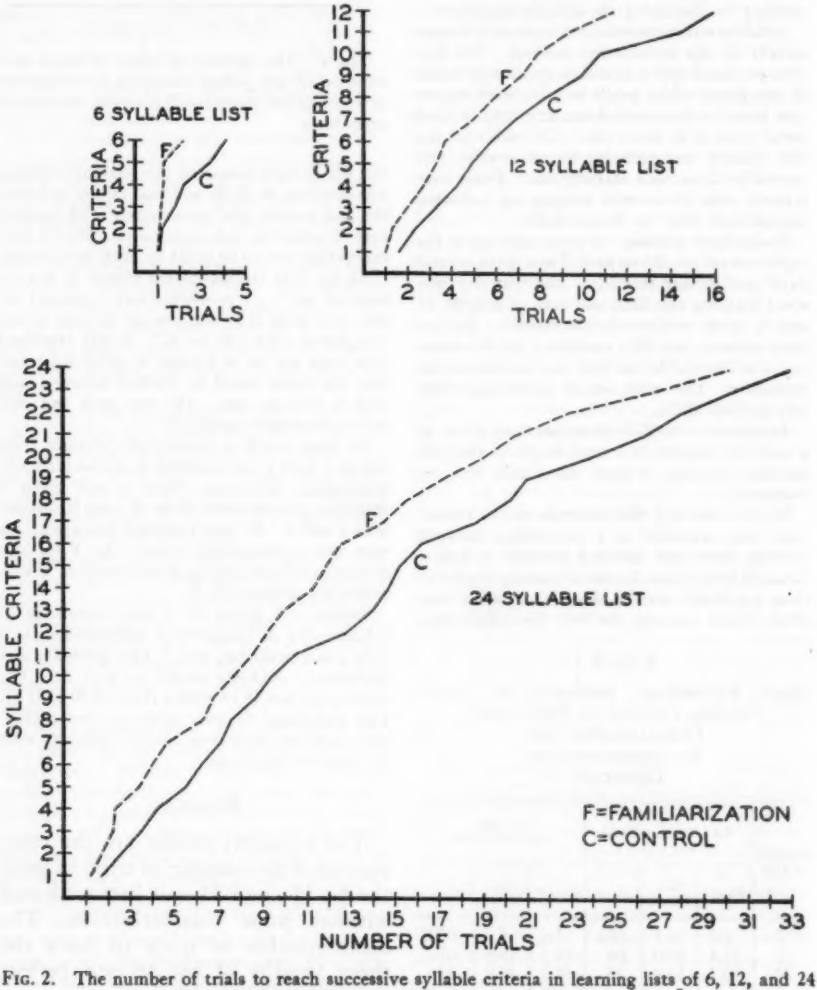


FIG. 2. The number of trials to reach successive syllable criteria in learning lists of 6, 12, and 24 syllables under familiarization (F) and control (C) conditions

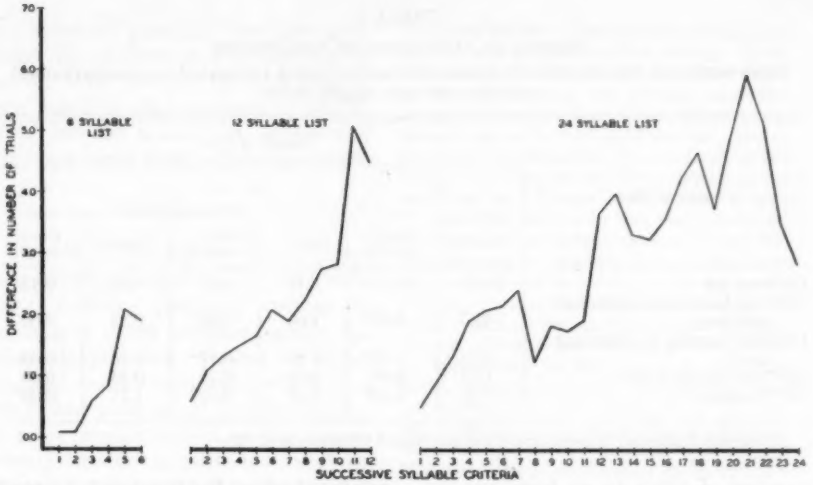


FIG. 3. The *difference* between familiarization and control procedures in the number of trials to reach successive syllable criteria

distortion into the length-difficulty relationship. The results for the criterion of two perfect recitations tend to indicate a somewhat greater effect of familiarization with greater length of list, but here also there is a linear relationship between length of list and difficulty with both procedures.

Another method of analyzing the results is to determine the influence of prior differentiation on the acquisition curves for each particular length. Figure 2 gives the acquisition curves with and without prior differentiation for the three lengths of list. The average number of trials to reach each syllable criterion is plotted. It will be seen that the criteria are plotted along the ordinate and the number of trials along the abscissa. This departure from the conventional use of the abscissa for the independent variable was employed to preserve the customary form of the learning curve. The effect of the familiarization will be seen to be most marked for the 6-unit lists. Calculation from the individual records shows that two-thirds of the *S*s were able to get five of the six syllables correct on a single trial with the prior familiarization but only one out of the 12 *S*s made a similar score under control conditions. With the 12-unit lists, half of the *S*s were able to get three syllables correct on the first trial with the prior familiarization, but none of them reached this criterion under the control conditions.

To determine more clearly at what stage of learning the advantage of prior familiarization occurs, it is useful to compare the *rates* of learning for the two procedures throughout the course of learning. This comparison is made in Fig. 3. The values presented are obtained by computing the *difference* in number of trials required to reach successive criteria under familiarization and control procedures. The amount by which the familiarization procedure is superior to the control is plotted at successive criteria of learning. Wherever the rates of learning are equal (i.e., when the increments in

TABLE 2

EFFECTS OF FAMILIARIZATION UPON ERRORS

Mean number of different kinds of errors under familiarization and control (nonfamiliarization) conditions for three lengths of list.

| Source of Error | Length of List | | | | | |
|---|---|---|---|---|---|---|
| | 24 | | 12 | | 6 | |
| | Control | Familiarized | Control | Familiarized | Control | Familiarized |
| (1) Same list | 28.50 | 34.67 | 6.17 | 6.67 | 0.42 | 0.42 |
| (2) Prior learning (nonfamiliarized lists) | 4.67 | 0.92* | 1.08 | 0.00 | 0.25 | 0.00 |
| (3) Prior learning (familiarized lists) | 6.33 | 1.25* | 1.50 | 0.17* | 0.58 | 0.00 |
| (4) Prior familiarization | 1.17 | 0.25 | 0.08 | 0.25 | 0.33 | 0.00 |
| (5) "Invented" | 17.50 | 3.50* | 5.67 | 0.50* | 1.75 | 0.08* |

* Significant difference between control and familiarized conditions ($p < .02$).

number of trials to go from one criterion to the next are equal) for the two procedures the plot of the differences remains horizontal. A rise in the curve indicates a more rapid rate of learning at this stage for the prior familiarization procedure, and a fall indicates a more rapid rate under the control condition. It will be seen that despite some minor irregularities a higher rate of learning for the familiarization procedure is maintained throughout learning with the 6- and 12-unit lists. With the 24-unit list the rate of learning is higher under the familiarization procedure up to the criterion of 21/24 syllables correct. From this point until mastery (24/24) the rate is higher under control conditions, although there still remains a net difference in favor of familiarization.

*Analysis of errors.*—A further analysis of the data was made in terms of the number and kinds of errors made under the two conditions. All incorrect responses during serial learning were classified according to the following five categories: (*a*) Syllables belonging to the list being learned (intralist errors); (*b*) syllables belonging to a nonfamiliarized list previously learned by *S*; (*c*) syllables belonging to a familiarized list previously learned by *S*; (*d*) syllables included in a preceding familiarization procedure but not included in any serial learning; and (*e*) syllables not previously presented to *S* in any fashion during the experiment and, thus, "invented" by *S*. If a given error was repeated several times during a trial it was counted only once, unless two occurrences were separated by a different error. Only responses which were complete syllables were included in the analysis. These constituted 78% of all errors. The mean frequency of each type of error is presented for each condition and each length of list in Table 2. The difference between the familiar and nonfamiliar conditions in number of intralist errors is not significant for any length list. The number of invented errors is reduced significantly for all lengths of list when the material learned is previously familiarized. Similarly, familiarization reduces the number of errors from previous lists, both familiarized and not familiarized. When the contributions from these

two types of previous lists are combined, the difference is significant at the .01 level for the 12- and 24-unit lists, but falls slightly short of significance for the 6-unit list, where few errors are made under either condition.

## Discussion

The data bearing on the relationship between length of list and difficulty confirm previous studies in showing a rough proportionality between length and number of trials required for learning, and show that this relationship is not greatly affected by prior familiarization. What is now greatly needed is information about the relationship with lists of considerably greater length. Only in this way can the formulae of Thurstone (11) and Hull *et al.* (5) be tested adequately. The data now available covering the greatest range of lengths are those of Lyon (6). They do not appear to follow a simple formula, but show wavelike changes somewhat analogous to the plateaus and habit hierarchies found by Bryan and Harter (1). Unfortunately, Lyon's data are based on only a single *S* (himself) and more research employing a wider range of *S*s is needed to ascertain the length-difficulty relationship with adequate generality. Present data provide little information on individual differences, since most of the studies have been done with college students and other above-average adult *S*s.

The effects of prior familiarization are clearly seen to be an increased ease of learning, both in terms of errors and trials to mastery. In the case of the shortest list only about half the number of trials is required following familiarization as under standard conditions. The present results thus do not support those of Waters (12), who concluded that prior reading of lists of syllables does not facilitate learning. In his experiment three conditions were compared: Cond. I—The *S*s first read aloud three times the list to be learned. They then proceeded to the formal learning of the list. The serial order for learning was the same as for the initial reading. Cond. II—Same as Cond. I except that the order of syllables for learning was different from that during the initial reading. Cond. III—Learning without any prior reading of the list. Waters considers the comparison between Cond. II and III the crucial test for the effect of familiarization and found no difference in the number of trials to learn the lists under the two procedures. However, this comparison neglects the possibility that learning the syllables in a changed order may involve the unlearning of partly learned associative connections established during the initial reading with resulting associative interference. Thus, in Waters' experiment there was an opportunity for the factors of associative interference and familiarity to oppose and cancel one another, while in the present study the learning of associative connections during familiarization was precluded by using a constantly changing order of presentation during familiarization. A further difference between the two studies is in the amount of familiarization training given. In Waters' study familiarization consisted of only three prior readings of the lists to be learned, while in the present experiment between 30 and 70 presentations of the material were employed, the number depending upon *S*'s speed in reaching the familiarization criterion.

Our results, then, seem to revive the possibility that Robinson's Law of Acquaintance deserves further consideration. But if, as Robinson (9) suggests, there is some advantage in familiarity with the units to be associated which can be abstracted from the many associative connections into which they enter, what is the basis for its effect?

One possible interpretation follows from the type of "communication theory" discussed by Miller (8, Ch. 5, 10, 11). He defines the amount of information conveyed by an item as dependent upon the number of alternatives from which that item is chosen, and cites evidence that learning time increases with amount of information communicated. The effect of familiarization in the present study is to sharply reduce the number of alternative items from the range of all possible nonsense syllables to the limited

number to be learned. The amount of information conveyed by each syllable at the time of learning is thus greatly reduced and, accordingly, it would be expected that familiar (low-information) syllables should be learned more rapidly than the control material.

A second interpretation is suggested by Gibson (3) in her discussion of the learning of "predifferentiated" items. According to her formulation, the familiarization procedure would produce a clear discrimination among the units to be associated and between these and units learned in earlier lists. This would reduce both intra- and interlist interference and thus increase the speed of learning. However, the analysis of errors in the present study is not entirely consistent with this formulation, since there appeared to be no appreciable reduction of intralist errors under the familiarized condition, although there was the predicted decrease in interlist intrusions.

An hypothesis suggested by the present data is that in the serial learning of both familiarized and nonfamiliarized syllables there develop tendencies to make all three kinds of errors, *viz.*, intralist intrusions, interlist intrusions, and inventions. Familiarization enables *S* to recognize the last two types of responses as foreign to the list being learned and, thus, as incorrect, so that the tendencies to make such responses, although present, are suppressed. On the other hand, familiarization provides no basis for knowing the correct serial order of syllables in the list being learned, and errors due to misplacing of these syllables are made with about the same frequency under familiar and nonfamiliar conditions.

This hypothesis is substantiated by the results of an unpublished study by Sheffield (10). In that study syllables were familiarized by a procedure not involving the formation of associations between syllables and were then paired with nonfamiliar syllables in a paired-associates learning procedure. The results indicate that increases in the speed of learning and decreases in intralist intrusions are more closely related to the occurrence of familiar syllables in the response position than in the stimulus position.

Further research is needed to analyze more fully the role of stimulus and response differentiation in learning and the relationship of these phenomena to the broader problem of meaningfulness in learning.

## Summary

1. In the serial learning of nonsense syllables the process of becoming familiar with the syllable units may be distinguished from the process of learning the serial order of these units. For research in which the primary interest is in the latter process, it would be desirable to have some method of eliminating the former. In the present study an attempt was made to compare the relationship between difficulty and length of list under ordinary nonsense-syllable procedures with that under a procedure in which *Ss* became familiar with the syllables before learning them in serial order. In the design employed, 12 *Ss* learned lists of 6, 12, and 24 nonsense syllables both with and without prior familiarization of items.

2. The effect of prior familiarization with the syllables was to reduce the number of trials to mastery by a moderately constant amount for all lengths of list. In the case of the shortest lists this amounted to a reduction of approximately half the number of trials required by the ordinary procedure, while with the longer lists it amounted to a much smaller proportion. The present results on the

effects of familiarization are discussed in relation to the results of Waters (12) which showed no effect of prior reading of the material to be learned.

3. The number of trials to reach the criteria of mastery employed was an approximately linear function of length of list in the range of lengths studied. This held for both criteria of learning and for both familiarization and control procedures.

4. As measured by the difference in trials to reach each syllable criterion, the rate of acquisition following familiarization was higher than that of control material throughout learning for the 6- and 12-syllable lists. Prior familiarization also produced a superior rate of learning with the 24-syllable list up until the final eighth of acquisition, when the rate of learning fell below that under control conditions. The net advantage of familiarization was thus reduced somewhat by the time that the criterion of one perfect recitation was reached.

5. Analysis of errors indicated that familiarization did not reduce the number of intralist intrusions but did reduce the number of interlist intrusions and the number of erroneous syllables "invented" by *S*s.

6. The results are discussed in relation to Robinson's Law of Acquaintance (9), the generalization-differentition analysis of Gibson (3), and the "information theory" formulation by Miller (8).

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# AN EXPERIMENTAL NOTE ON A HULL–LEEPER DIFFERENCE

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By the "Hull-Leeper difference" we mean the rather considerable discussion which was touched off by Hull's paper on drive specificity in 1933 (2) and by a set of similar experiments reported in 1935 by Leeper (6). Guided by his conditioned reflex reinforcement theory Hull argued that "if internal conditions such as hunger and thirst are capable of becoming conditioned in such a way as to evoke ordinary action, albino rats, when all other elements in the stimulus complex are the same, should learn to take one of two alternative paths to a goal box when hungry and to take the other when thirsty" (2, p. 258). Leeper's investigations stemmed from his analysis of the learning process in terms of the cognitive concepts of Tolman (9) from which Leeper thought that "it naturally follows that an animal should have no difficulty in adapting its response immediately when its motivation is changed, provided its past experience has given it a basis for knowing that one response leads to one consequence and another response leads to a second consequence" (6, p. 12).

The design of Hull's maze (2) required the animals to enter the *same final alley* under conditions of hunger and thirst, and to perform the consummatory acts in the *same goal box.* Leeper's maze was designed as a single-unit **T** with extensions parallel to the stem of the **T**; thus, his animals did not use a common approach to the goal box, and in his design the consummatory responses occurred in different boxes. On a correct run the procedures of both Leeper and Hull permitted immediate reinforcement. On the incorrect runs the procedures differed in that Hull's rats were not allowed to enter the goal box containing the "wrong" incentive, whereas Leeper's animals were not prevented from so doing. Leeper argued that, consequently, one of his animals, when satiated for food and entering the food box, should "learn something that would help it to change its behavior on the following day" (7, p. 103); but that Hull's animal, meeting the closed door, "should learn something which should hamper its performance on the following day" (7, p. 103).

In view of the fact that Leeper's animals mastered the problem in about one-twentieth of the time required by Hull's animals, Leeper's conclusions were as follows:

> This set of experiments permits an unequivocal answer to the question of whether motivation can exercise a directive function in relation to the utilization of habit. . . . These experiments therefore, support the general idea which Tolman has emphasized, that habits . . . are to be described rather as knowledge that such and such behavior leads to such and such consequences . . . (6, p. 36). It appeared that these results were caused by the fact that, on their incorrect runs, the rats met a closed door at the end of the path, rather than an end box containing the goal-material not required at the time . . . (6, p. 38).

The results of several subsequent experiments (4, 5, 8) which had quite

crucial theoretical significance for Leeper's position indicated that the lesson of the experiments described in a previous paragraph was not quite so "unequivocal" as Leeper had hoped; indeed, the evidence was powerful enough to compel a revision of his formulation (7).

We have already called attention to the fact that Hull's maze provided a single common path into the goal box, and that the consummatory responses were executed in the same goal box. It is the significance of this aspect of Hull's design for reinforcement theory with which we are here concerned. Under these conditions the two different $r_g$'s (3) which are elicited (one under hunger and one under thirst conditions) become conditioned to *identical* stimuli, a fact which is conducive to considerable competition of the $r_g$'s and a consequent slowing down of the learning process. Furthermore, if we consider that there are two behavior chains to be supported (the "incorrect" turn followed by retracing, and the "correct" turn leading more immediately to the final common path) it appears to us that the identical stimuli of the final common path will provide enough secondary reinforcement for the "wrong" chain to make a greater number of trials necessary than would be the case if the stimulus field were different in the final common path. If this theoretical position is sound it entails the following consequences: If, in a learning situation involving differential habituation to two different drives (for example, hunger and thirst)[1] in a design containing a final common path, the stimulus aspects of this path are changed for the two conditions of drive, the learning should be facilitated, there being the additional proviso that the stimulus field in which the consummatory responses are performed be also different for the two conditions. The investigation reported in this paper was designed to test this hypothesis.

[1] In view of Heron's (1) study it might be questionable to speak of "habituation to two drives"; rather, he suggests that the animals might be reacting to "full versus partially empty stomachs." However, Heron does not question the presence of *differences in internal organic stimuli* which become conditioned.

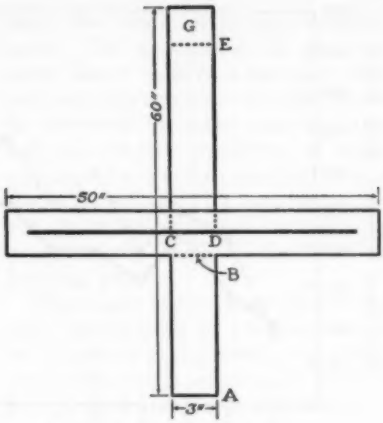


FIG. 1. Diagram of the maze

## METHOD

*Subjects.*—The *S*s were 9 male and 8 female albino rats approximately 90 days of age which had not previously been subjected to any experimental procedures. Four males and four females constituted the experimental group, and five males and four females provided the necessary controls.

*Apparatus.*—The maze employed is shown in Fig. 1. The runway was 3 in. wide and 3 in. high. The floor and sides were of wood; the top was a coarse-mesh screen. At *A* was a wooden door, hinged on the side, which could be opened and closed by *E*. At *B* was a screen door hinged at the top, which could be dropped by manipulation of cords. At *C* and *D* were doors similar to that at *B*. At *E* was a door which slid in vertical grooves. The entire maze was painted black. A white insert was made of rough cardboard; this insert could be fitted into the final common path for the purpose of securing the difference in the stimulus field already outlined. It extended from the junction at *C* and *D* to the goal com-

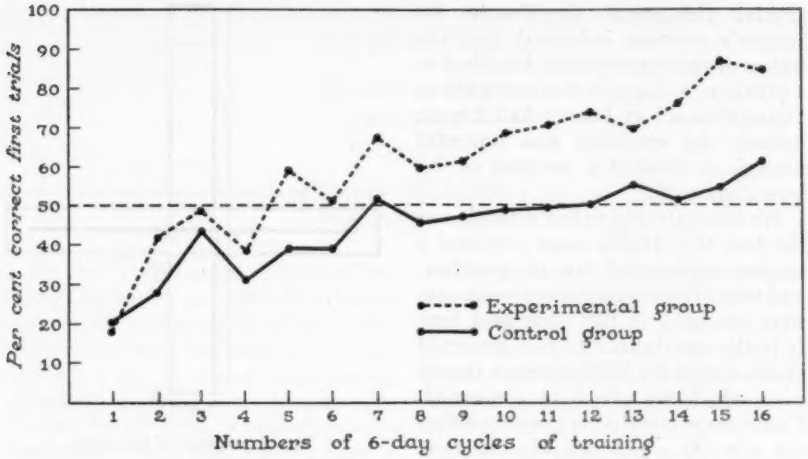


FIG. 2. Percentage of correct choices at the first trial of each experimental day

partment. Two goal boxes were constructed which were identical except that one was painted flat white on the inside.

*Procedure.*—The *S*s were habituated to handling, after which they were trained to run a straightaway with food and with water as incentives. With water as incentive, the white insert and goal box were used with the experimental group. After 12 days of such running in the straightaway the experiment proper was begun. Both groups were motivated on half the occasions by 23-hr. food deprivation and on half the occasions by 23-hr. water deprivation, a randomization procedure being used to assign deprivation to a specific day. The *S*s were satiated for food or water by being fed or watered for approximately one hour before the daily training period. Slightly moistened Purina Laboratory Chow was the food used. Three runs were given each *S* in close succession on each of six working days of the week. On hunger days the passage at the right was blocked at *D*, and on thirst days at *C*. Under these conditions it is difficult to see how results in the theoretically required direction (should such occur) could be interpreted as the result of having learned "where the goal-material not desired that day was to be found" (6). The control *S*s ran to a black compartment; the experimental group ran to this same box on hunger days, but on thirst days the white cardboard alley was inserted in the final path and the white goal box was substituted for the black one. A standard water bottle was suspended through a hole in the mesh top of the box. When *S* started in the wrong direction, but turned back in the right direction, the trial was tallied as an error.

## RESULTS AND DISCUSSION

Since, as Hull (2) pointed out and as Leeper (6) agreed, the first run of each day is the significant indicator of an association of the correct path with the internal drive stimulus, the percentages of correct first runs were computed for each group as a whole over cycles of six days, and are shown in Fig. 2.

It is obvious that the performance of the experimental group beginning with the second week is consistently better than that of the control group. Application of the formula for the significance of differences between proportions to the first runs of the final six-day cycle indicates the difference between experimental and control groups to be significant at the .05 level. The curves of the two groups never cross after the first six-day cycle. It seems appropriate to argue, therefore, that the theoretical analysis has led to the manipulation of a relevant experimental variable which has per-

mitted the prediction of results in this investigation.

Notwithstanding the fact that it was a theoretical reinforcement analysis which led to this prediction, it cannot be maintained that cognition theory would be unable to account for these results. On the other hand we do not find it very embarrassing for reinforcement theory that the efficiency of learning under Leeper's conditions is so much greater than the efficiency under our conditions or Hull's; for we regard it as one thing to show that a different *maze design* produces efficiency and quite another to show that this efficiency is derivable from the theoretical system. Although crucial experiments are quite rare in psychology, we suggest that it might be more nearly crucial to remain within one maze design; for example, if, within Hull's design, one group of *Ss* were permitted to enter the goal box under the "wrong" incentive (as Leeper suggested) with the final common path identical under hunger and thirst, and another group, *not* permitted to do so, were run under conditions of *different* stimulus fields for hunger and thirst (as in our experiment), we would expect the *Ss* in the second group to learn more efficiently than those in the first group.

## Summary

The problem was that of an experimental analysis of the phenomenon of drive specificity in the context of the Hull-Leeper investigations. It has been shown that a reinforcement analysis has permitted identification of relevant and somewhat neglected aspects of the stimulus field; and of special theoretical significance, in the development of this exposition, has been the factor of a final common path. The application of reinforcement theory to such a learning situation not only accounts for results thus far reported by other experimenters, but permits the prediction of consequences of a specified manipulation of the stimulus field. These results are not, however, sufficient for definitive rejection of a cognitive theory of the learning process.

The results of this investigation support the findings in previous studies to the effect that animals are able to learn differential responses in the context of food and water deprivation.

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# RETROACTIVE FACILITATION AND INTERFERENCE IN PERFORMANCE ON THE MODIFIED TWO-HAND COORDINATOR [1]

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This paper is one of a series dealing with facilitation and interference in the performance of complex motor tasks. It is concerned specifically with retroactive effects in performance on the Modified Two-Hand Coordinator.

The experimental procedure that has been used in several studies to be cited, and also in the present one, is as follows. The *Ss* are trained on a complex motor task. This constitutes original learning (OL). They next receive training on basically the same task, except that now the directions of movements of the controls are exactly opposite to those originally learned. This is the interpolated learning (IL) phase. Finally, they relearn (RL) the original task. The primary purpose of the studies has been to observe the proactive changes occurring during IL and/or the retroactive changes occurring during RL. The experimental variables have usually been the amounts (or levels)[2] of OL with amount of IL held constant or the amounts of IL with the amount of OL held constant. The relative strengths of the facilitative and interfering response tendencies under various conditions of practice are estimated, respectively, from gains and losses in performance.

One study, reported by Lewis, McAllister, and Adams (1), dealt with differences in performance on the Mashburn apparatus, during IL and RL, resulting from different amounts of OL, with the amount of IL held constant. Various amounts of OL on the standard task all resulted in proactive facilitation as well as proactive interference which persisted through several trials. The estimated amounts of proactive facilitation and interference were not clearly shown to be positively related to the amount of OL, although the evidence was equivocal. Retroactive interference effects of large magnitude and of persistent character were found for those groups receiving large amounts of OL and IL. Retroactive facilitation was distinguishable in a group which received small amounts of OL and IL and was thought to be present in all groups, its effects presumably being obscured by the effects of relatively large amounts of retroactive interference.

McAllister (2) used a similar design with the Modified Two-Hand Coordinator. She varied level (instead of amount) of OL while holding constant the level of IL. Positive relationships were found between the magnitude of the retroactive effects and the level of OL although "overlearning" induced less retroactive interference than lesser high levels of OL. No clear relationships between level of OL and amounts of proactive facilitation and interference were observed.

McAllister and Lewis (3) were concerned with differences in initial RL performance on the Mashburn apparatus following different amounts of IL, with OL held at a fixed amount. In general, the results indicated that the amount of retroactive interference depended upon the amount of IL.

Shephard (4) using the Mashburn apparatus, held the level of OL constant, varied the level of IL, and analyzed performance over 25 RL trials. The results indicated that retroactive interference predominated during the early RL trials. However, the amounts of retroactive interference and of retroactive facilitation were both directly related to the level of IL. Consequently, there was a "cross over" of the performance curves

[1] This work was done under Contract Nonr-166(00) between the State University of Iowa and Special Devices Center, Office of Naval Research.

[2] The term "amount of learning" is commonly used when practice continues for some specified number of trials, and the amount of learning is the number of trials. In contrast, the term "level of learning" is used when *S* continues practicing until a certain level of performance has been attained. In this case, level of learning is the prescribed level of performance.

after eight or ten RL trials, with the groups originally displaying the larger amounts of interference coming to display the larger amounts of facilitation.

A recent study by Smith (6), using the SAM Complex Coordination Test, Model D, confirmed Shephard's findings on the "cross over" effect.

The primary purpose of the present study was to determine the effects of different *levels* of IL, with the level of OL held constant, on retroactive facilitation and interference, when practice occurred on the Modified Two-Hand Coordinator. An additional objective was to obtain results on the Coordinator to be compared with those already available on the Modified Mashburn Apparatus.

## Method

*Apparatus.*—The Modified Two-Hand Coordinator was used. A detailed description of it appears elsewhere (5), and only the more basic features will be presented here.

The apparatus provides a target, ¼ in. in diameter, which revolves once per minute through the same irregular pathway. The *S*'s task is to turn two lathe-like handles in such a way as to keep the button, also ¼ in. in diameter, on top of the moving target.

A standard task, used for OL and RL, and a reversed task, used for IL, are available on the apparatus. The standard task requires clockwise turning of the right handle to move the button to *S*'s right, and counterclockwise turning to move it to the left. Clockwise turning of the left handle moves the button toward *S* while counterclockwise turning moves it away. For the reversed task, the handles must be turned in exactly opposite directions to obtain the above movements of the button. The target's pathway is the same for both tasks. One complete revolution of a handle moves the button ¼ in.

A trial lasts for the 30-sec. period during which the target revolves nearest *S*. The remaining 30 sec., during which the target completes its full cycle, comprises a rest pause. The *S* is instructed to return the button to the starting position after each 30-sec. trial and wait for a ready signal (a light) to indicate that the next trial is about to begin. The ready signal comes on 5 sec. before the start of a trial.

Three measures of performance are obtained on each trial: (*a*) The amount of time the button is kept on the target, recorded in .01-sec. units with a Standard Electric timer; (*b*) the number of incorrect responses made on each handle, one error being recorded on a Veeder-Root counter for each separate turning of a handle that moves the button away from the target; (*c*) the length of time *S* persists in making an incorrect response, recorded for each handle with a Standard timer.[3]

*Subjects.*—Sixty female students from a class in introductory psychology served as *S*s. They volunteered to participate but received two bonus points for each experimental period.

*Experimental design.*—There were five experimental groups and one control group, each composed of ten *S*s. All groups were first given practice on the standard task, and each *S* practiced until she achieved an OL performance criterion of 40 sec. on target during *two consecutive trial periods* each 30 sec. in length. This meant that the single-trial criterion was 20 sec., on the average. The two-trial criterion was employed in an attempt to secure a more uniform and reliable level of OL. A maximum of 40 trials per day was given. When *S* met the two-trial OL criterion, training on the task was terminated with practice on the reversed task not starting until the following day.

A two-trial performance criterion was also used for the experimental groups during IL when the reversed task was being learned. Group F, the control group, went without practice for a period of three days—the number of practice days required, on the average, for the experimental groups to attain the prescribed IL levels. The experimental groups attained the two-trial criterion of 24, 32, 40, 48, and 56 sec. on target in the case of Groups A, B, C, D, and E, respectively. This meant single trial average times for the five groups of 12, 16, 20, 24, and 28 sec.

Relearning was uniform for all six groups. They were given a total of 40 trials, 25 trials on one day and 15 on the next. The experimental groups began RL on the day following the attainment of the IL criterion while the control group began RL on the fourth day following the completion of OL.

As already indicated, the trials were all 30 sec. in length. Rest pauses of 30 sec. were interspersed among the trials with longer rests of either 2 or 5 min. being introduced after blocks of five or ten trials.

Instructions were read to *S* at the beginning of the first day of training and also at the points of shifting from one task to another. The *S*s

[3] It was not desired in the present study to deal with right- and left-hand errors separately, so the error measures (number of errors and persistence of errors) were combined for the two control handles. Throughout the paper, any reference to error measures will be to the totals for the two controls.

TABLE 1

MEANS OF NUMBER OF TRIALS TO THE OL CRITERION, MEANS OF TIME-ON-TARGET SCORES ON SPECIFIED TRIALS, AND MEANS OF DECREMENTS IN TIME ON TARGET FROM THE LAST TWO OL TRIALS TO THE FIRST TWO RL TRIALS

| Group | Two-Trial Criterion for IL (In Sec.) | Trials to OL Criterion | Means of Time on Target (30-sec. Trial) | | | | |
|---|---|---|---|---|---|---|---|
| | | | Last Two OL Trials | First IL Trial | First RL Trial | RL Trials 1 and 2 | Mean Decrement |
| F | No IL | 42.6 | 20.77 | | 17.15 | 18.11 | 2.66 |
| A | 24 | 41.7 | 20.89 | 3.33 | 11.84 | 13.36 | 7.53 |
| B | 32 | 53.8 | 21.04 | 2.56 | 9.14 | 9.87 | 11.17 |
| C | 40 | 35.8 | 20.71 | 3.06 | 7.37 | 8.14 | 12.57 |
| D | 48 | 45.6 | 20.84 | 2.21 | 4.91 | 6.84 | 14.00 |
| E | 56 | 39.4 | 20.84 | 2.87 | 5.20 | 5.95 | 14.89 |

were urged to do their best but were never informed concerning their scores.

## RESULTS AND DISCUSSION

It was anticipated that the decrements in proficiency on the standard task at the outset of RL would be directly related to level of attainment on the reversed task, i.e., that Group E would display the greatest decrement while Group F would display the smallest. It was further anticipated that interference would dissipate during the relearning trials and that the effects of retroactive facilitation would begin to appear during the later stages of RL, provided that differential amounts of facilitation developed during IL. Retroactive facilitation would be expected to consist of appropriate response tendencies, acquired during IL, which could be utilized in the performance of the standard task. These appropriate tendencies would not dissipate during relearning so their effects would become apparent subsequent to the dissipation of the interfering response tendencies. Differential losses in proficiency among the groups would arise from differential amounts of interference. On the other hand, differential gains in proficiency, either early or late in relearning, would arise from differential amounts of facilitation.

*Time on target.*—Inasmuch as time-on-target criteria were used in controlling levels of performance during OL and IL, the most immediately meaningful analysis of the results utilized the time-on-target scores. The comparability of the six groups on the last two trials of OL is seen in the fourth column of Table 1 where the means of time on target per trial are presented. As would be expected, the six means are virtually identical. Another way of showing the comparability of the OL performances of the six groups was to check on the number of trials required to attain the OL criterion of 40 sec. on target on two consecutive trials. The means of the number of trials for the six groups are shown in the third column of Table 1. The $F$ ratio, based on the between-groups and within-groups variance estimates, was 2.06. With 5 and 54 $df$, a value of $F$ greater than 2.06 would be expected more than 5% of the time in random sampling if the null hypothesis were true. It was reasonable to assume, therefore, that the groups were essentially equal in rate of acquiring proficiency on the standard task.

The time-on-target scores of the

five experimental groups on the first trial of IL could be examined as a further check on their comparability. The means are given in the fifth column of Table 1. Simple analysis of variance was employed to test the null hypothesis. The resulting $F$ ratio was .44. With 4 and 45 $df$, [this] falls far

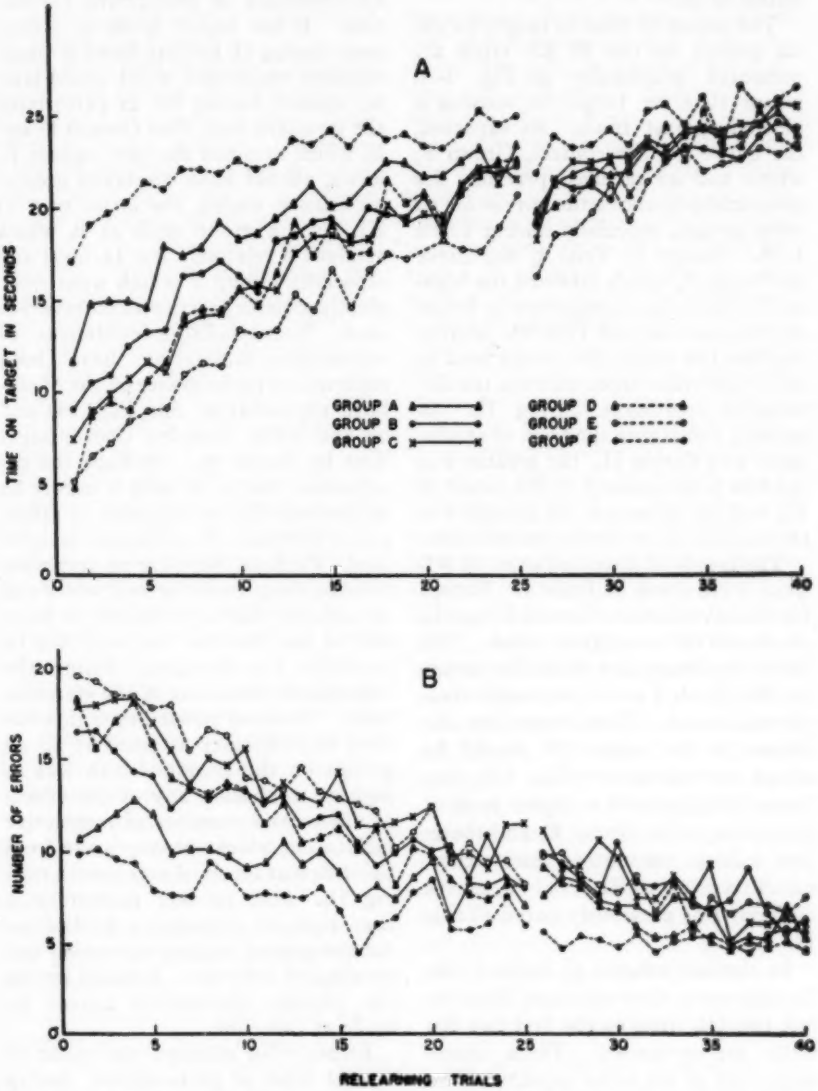


FIG. 1. Means of time on target (upper graph) and means of number of errors (lower graph) for the six groups on the 40 trials of relearning

below the 5% level of confidence. Thus, the five experimental groups could be assumed to have equal ability to perform the reversed task at the outset of IL.

The means of time on target for the six groups on the 40 RL trials are presented graphically in Fig. 1-A, where time on target in seconds is plotted against trials. As expected, the curve for the control, Group F, which had no reversed practice, lies conspicuously above the curves for all other groups, especially during Trials 1–25. Except at Trial 1, the curve for Group E, which attained the highest IL level, lies conspicuously below all other curves until Trial 30. During the first few trials, the curves tend to fall in the order expected from the differential treatment during IL. In general, the higher the level of attainment was during IL, the greater was the loss in proficiency at the outset of RL and, by inference, the greater was the amount of retroactive interference.

The means of time on target on RL Trial 1 are listed in Table 1. Except for the inversion for Groups D and E, the means fall in a regular order. The inversion disappears when the means for RL Trials 1 and 2 in combination are calculated. These means are also shown in the table. It should be noted, by reference to Fig. 1-A, that Group D displayed a higher level of proficiency than Group E, and therefore a lesser amount of interference, on all trials up to RL Trial 30, except on Trial 1 as previously noted (Table 1).

In the last column of Table 1, the decrements in time on target from the last two OL trials to the first two RL trials are presented. These decrements fall in the order expected from the differential treatment during IL and reflect the relative amounts of interference resulting from the different IL levels.

There are no detectable signs in Fig. 1-A of the functioning of differential amounts of retroactive facilitation. If the higher levels of attainment during IL had produced stronger response tendencies which could later be utilized during RL in performing the standard task, then Groups D and E, which attained the two highest IL levels, should have displayed greater proficiency during the latter part of RL than a group such as A which attained a relatively low IL level and especially Group F which went completely without practice on the reversed task. Unmistakable evidences of retroactive facilitation have been reported for performance on the Mashburn apparatus by Shephard (**4**) and on the SAM Complex Coordination Test by Smith (**6**). Perhaps the coordinator task is of such a nature as to preclude the development of differential amounts of retroactive facilitation. Perhaps there are no responses to basic elements of the task which can be acquired during extended IL practice on the reversed task and then be available for elicitation during the subsequent relearning of the standard task. A second possibility is that the level of proficiency attained by all six groups on the standard task was so high as to obscure any of the effects of differential amounts of retroactive facilitation which actually arose from the different levels of attainment during IL. Still another possibility is that signs of retroactive facilitation did not appear because relearning was terminated too soon. A choice among the various alternatives cannot be made at this time.

*Errors.*—No attempt was made to control level of performance, during either OL or IL, in terms of the number of errors or persistence of errors

per trial. It was not anticipated that the groups would necessarily be comparable with respect to these measures, but the error scores were thought to be potentially useful in analyzing the retroactive effects.

The first step was to determine whether the groups could be regarded as basically the same in their tendencies to make incorrect responses, so the differences among the means of number of errors for the six groups on the last two trials of OL were evaluated. Simple analysis of variance was employed. The *F* ratio, based on the between-groups and within-groups variance estimates, was 1.06. With 5 and 54 *df*, this value falls far below the 5% level of significance. It was assumed, therefore, that the six groups were basically alike in their tendencies to make errors.

The means of number of errors for the five experimental groups on the first IL trial were also subjected to scrutiny. Simple analysis of variance was again used. The obtained value of *F* was .29 which, with 4 and 45 *df*, led immediately to the retention of the null hypothesis.

Because the groups seemed comparable in their tendencies to make errors, the means of number of errors on the RL trials could be more or less directly evaluated for additional information they might give on retroactive effects. The data are presented in Fig. 1-B where mean number of errors is plotted against trials. On RL Trial 1, the means of the six groups fell in the order that might have been expected from the different levels of IL. It will be noted, however, that the curves, especially for Groups B, C, and D, overlap and crisscross. Nevertheless, there are trends in the curves which confirm, in a general way, the conclusion that the amount of retroactive interference was dependent upon the level of attainment during IL. It is not until almost the end of relearning that the means of number of errors become virtually identical for all six groups. As in the case of the time-on-target data, the means of number of errors do not indicate any differential functioning of retroactive facilitation.

A study was also made of the measures of persistence of errors. These were time scores, and revealed the tendency of *S*s to persist in errors after they had been committed. It was not known in advance whether the persistence scores would be consistent enough or reliable enough to be useful in revealing either facilitation or interference. An analysis was made of the mean persistence scores for the six groups on the last two trials of OL, the differences among the means being tested for significance through simple analysis of variance. The value of *F* was 2.73 which, with 5 and 54 *df*, falls between the 1% and 5% levels. Simple analysis of variance, when applied to the persistence scores on the first trial of IL, yielded an *F* ratio of .82 which with 4 and 45 *df*, is far below the 5% level. The means for the six groups on the 40 trials of RL were computed and plotted. The trends of the resulting curves were similar to the trends of the curves based on means of time on target and means of number of errors. Since the persistence data for relearning provided no additional insight into the processes of interference and facilitation, it seemed a waste of space to include a plot of them.

## Summary

Six groups of ten female *S*s first attained a prescribed level of proficiency on the standard task of the Modified Two-Hand Coordinator. The

five experimental groups then attained different levels of proficiency on the reversed coordinator task, while the control group went without practice of any kind. All groups relearned the standard task under uniform conditions.

The results showed, as expected, that the amount of retroactive interference as reflected by losses in proficiency during relearning, depended upon the level of proficiency attained on the reversed task during IL.

There were no signs that differential amounts of retroactive facilitation were developed during IL. This finding, which was contrary to expectations, was interpreted as indicating that (*a*) the coordinator task is such as to preclude the development of appreciable amounts of retroactive facilitation, or (*b*) the design of the experiment was not suitable for revealing differential facilitative effects.

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# LISTENING TO ONE OF TWO SYNCHRONOUS MESSAGES

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The general situation of using two or more communications channels at once or in alternation gives rise to many specific problems. One of these, the difficulty of speaking and listening simultaneously, has already been studied (2). Another is the varying difficulty of identifying a message as important when it arrives while other messages are already in progress: this is being examined elsewhere (8). The present report is concerned with the possibility of answering one of two messages which start at the same point in time, but one of which is irrelevant.

## Procedure

The general technique used has been described more fully in the previous paper (2). Briefly, *S* was required to answer in a strict radiotelephone procedure a series of Yes-No questions about a visual display.

The short form of procedure was used throughout the present experiments, that is, each message began with only two call signs linked by the word "from." An example is "S-1 from G.D.O. Is there a heart on Position 1? Over," to which the answer would be "G.D.O. from S-1. Yes, over." The recording equipment used on this occasion was a Ferrograph Type C tape recorder. This machine is of high fidelity compared with many circuits used in practice. The one used had a response flat to 8 kilocycles/sec., at output to the speaker. The built-in speaker was used at about 1 m. from *S*. The intensity of playback, as before, was kept at a mean of about 75 db.

All *S*s received a training period on the task on one day, and on the second day began the experiment proper. Groups I, II, III, and IV then heard two successive series of messages in which from time to time two voices spoke simultaneously. One of the two voices was addressing S-1, while the other was addressing S-2, S-3, S-4, S-5, or S-6. The recordings were made so that both messages started together: the same two voices were used both in practice and on the experimental day, one being known throughout as G.D.O. and one as Turret. These voices were both those of young men, the accent being that sometimes known as "standard British" (BBC). The groups of *S*s were given different instructions as follows:

Group I (12 *S*s) was instructed to answer the message for S-1 and to ignore the other. Both series were performed under these conditions, and no cue other than the auditory one was given to the correct message.

Group II (12 *S*s) was instructed on one run that G.D.O. was always to be answered regardless of the call sign preceding the message. On the other run this group was provided with the names G.D.O. and Turret as part of the visual display, and immediately before each pair of messages began, *E* indicated the particular voice to be answered. The voice indicated was always the one which was in fact addressing S-1 and *S*s were informed of this fact. The two conditions were given in the reverse order to alternate *S*s.

Group III (10 *S*s) were on one run given the same instructions as Group I, and on the other run had the correct voice indicated by *E*. Unlike Group II, however, they did not have the voice indicated until the two synchronous messages had reached the "over" stage. The two conditions were given in the reverse order to alternate *S*s.

Group IV (8 *S*s) always had the correct voice indicated, but in one run before the two voices began, as in Group II, and in the other run as they were finishing, as in Group III.

Group V (12 *S*s) heard only one of the two experimental recordings used on the other four groups, and heard it under the same conditions as Group I. Two fresh recordings were also presented to this group. In one of them, one voice was addressing S-1 as before, while the other was addressing T-2, T-3, T-4, T-5, or T-6. Thus, the simultaneously received call-signs differed more markedly than in the recordings used on the other groups. In the third recording for this group, this greater differentiation of call signs was retained and in addition both voices repeated the call-sign portion of their re-

[1] This work was done while the author was receiving a grant from the British Medical Research Council, and it is a pleasure to acknowledge the assistance given by the Royal Navy both in supplying *S*s and in providing the technical equipment.

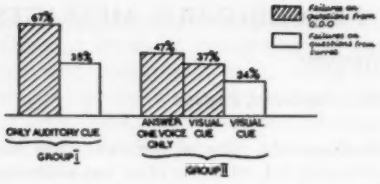


FIG. 1. Effect of non-auditory cue

spective messages twice over, thus: "S-1 from G.D.O., S-1 from G.D.O." The order of presentation of the recordings was varied on the latin-square principle, and before each run each *S* was informed of its nature.

The *S*s were all Naval Ratings under the age of 32. All but nine were Fleet Air Arm Ratings: these nine were of other trades. Four of the latter were in Group IV, one in Group V, and four in Group II. They did not appear to differ in performance from the Fleet Air Arm Ratings.

## RESULTS

*Effect of nonauditory cues to the correct messages.*—The results of Groups I and II are shown in Fig. 1. It will be seen that, while the over-all proportion of failures to answer the correct message correctly is 52%, there are more failures on one voice than there are on the other. This difference is highly significant ($t = 4.05$; $p_{.05} = 3.11$). It is also of interest that the correct message was answered *wrongly* in only 4% of cases, while the incorrect message was answered rightly in 16%. There was no significant improvement with practice ($t = 1.46$, $p_{.05} = 2.20$).

Group II was required on one run to answer always the more difficult voice and on the other one to change from one voice to the other in obedience to a visual signal. The results shown for the former condition, in Fig. 1, apply only to those messages in which the more difficult voice was correct under either condition, since on the remaining messages the two conditions would not be strictly comparable. The difference between the two conditions was insignificant ($t = 1.26$; $p_{.05} = 2.23$). The second series of messages was performed better than the first ($t = 2.37$). It appeared that *a definite knowledge of the voice to be answered improved performance*, since the difference in performance between Group II and Group I is significant. Pooling the data for the more difficult voice under both conditions in Group II, $t = 3.39$ for the difference from the performance of Group I on the same messages, while the .01 level is 2.82. For the less difficult voice the distributions are somewhat distorted, and the ranking method of Whitfield (11) was employed. The S-Continuity Correction/S.E. of S = 2.57 while the .02 level is 2.33.

*Effect of temporal position of the nonauditory cues.*—The results from Groups III and IV, shown in Fig. 2, make it clear that delaying knowledge of the correct voice until the message is almost completed largely abolishes its usefulness.

Group II, it will be remembered, on one run received only the auditory indication of the correct message, and on the other run, received a visual signal like that given to Group II but delayed to the end of the message. There was no significant difference between the two conditions

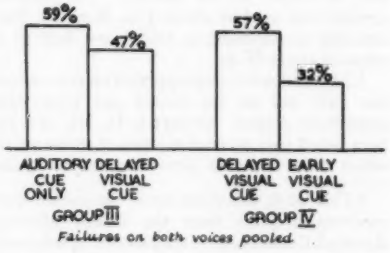


FIG. 2. Delay in giving nonauditory cue

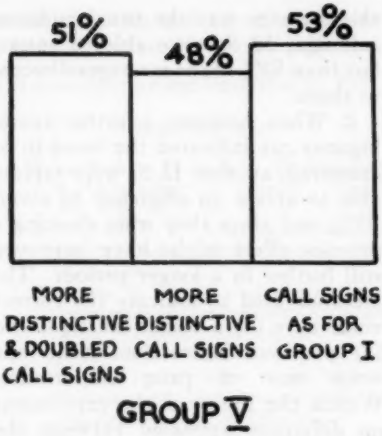


FIG. 3. Variation in distinctiveness of call sign

($t$ = 1.77; $p_{.05}$ = 2.31). No practice effect appeared between the two runs ($t$ = 1.0).

As the former value of $t$ is quite large even though insignificant, the data from Group IV form a useful check on the above conclusion. Here the temporal positions of the visual signal are compared directly and give a significant difference even on eight *S*s ($t$ = 2.53; $p_{.05}$ = 2.45). Once again there is no practice effect ($t$ = .63).

*Effect of variations in the auditory call sign.*—Group V compared the call signs previously used with a more differentiated call sign and with repeated call signs. No difference appeared between these conditions. The actual results obtained are shown in Fig. 3. The variance between the different call-sign conditions is slightly less than the remainder, so that remainder variance/treatment variance equals 1.41, while the .05 level is 19.4. There was no significant "practice" effect, the relevant $F$ being 2.68, while the .05 level is 3.49.

## DISCUSSION

From the practical point of view, these experiments show that there is a possibility, when two messages arrive simultaneously, of *identification of the message to be answered becoming a more serious problem than the understanding of it once identified.* Limitations on this conclusion are:

*a.* With poor quality communications equipment understanding of one voice in the presence of others may become harder. The problem of identification is only worth considering after the channel concerned has been improved to the point at which voices are discriminable.

*b.* If the competing voices differ in spatial localization, the understanding of a selected voice will probably be improved (5), but it is not known how identification will be affected. Further research is planned on this point.

*c.* These results apply only when two call signs occur together, and one and only one is to receive attention. For other possible conditions in which a call sign may arrive reference must be made to the results of Poulton (8).

More generally, the present case is an instance of selection in perception (attention). Since the visual cue to the correct voice is useless when it arrives towards the end of the message, it is clear that a process of discarding part of the information contained in the mixed voices has already taken place. From the closeness of the errors in Group I to 50% it seems possible that one of the two voices is selected for response without reference to its correctness, and that the other is ignored. It is understandable, then, that simultaneous auditory call signs should be ineffective: if one of the two voices is selected (attended to) in the resulting mixture there is no guarantee that it will be the correct one, and both call signs cannot be perceived at once any more than both messages can be received and stored till a visual cue indicates the one to be answered.

The fact that such a process of selection can be shown with auditory material is of some interest. The role of selection in visual performance has often been emphasized; an experiment by Chapman (3) presents an almost exact parallel to the present results, in the visual field. But it is arguable that visual selection is a matter of sensory adjustment, particularly of fixation. This argument is frequently raised, since it would obviously simplify theories of learning to suppose that all stimulation which

actually affects the receptor field is equally likely to be learned (1). A demonstration of the role of attention in hearing has been carried out (7) in order to show that selection takes place within the nervous system proper, but, as has been pointed out by Freeman (4), this inference is not wholly convincing. Since the situation is purely one of reacting to a single stimulus, reaction time being taken as an index of attention, such factors as the degree of tension in the tensor tympani and the spatial orientation of the ears may be present. The difficulty is not wholly overcome in the further work of Mowrer (6), though it is made to seem improbable. In the present work, on the other hand, since the two voices are not spatially separated and since both are presented simultaneously, the known processes of aural adjustment are inadequate to account for selection. It does not, of course, necessarily follow that discarding of information (that is, neglect of certain stimulus characteristics) occurs in all situations, particularly those involving lower organisms. But it does mean that selection is sometimes present and must not be neglected.

The importance of perceptual selection is not limited merely to the complication of learning theory previously mentioned. It has been shown recently that it may affect features of learning commonly regarded as basic, such as the curve of difficulty against ordinal position in learning serial material (9). And it would seem that in any continuous performance it may be insufficient merely to ensure the presence of essential signals at any one stage: it is also necessary to ensure that the operator is prepared in advance to select the signals when they appear. Thus in the case of auditory communications with a visual call-up the amount of information transmitted by the engineering system is identical whether the visual signal precedes or follows the mixture of messages, information here being measured in the manner usual for such systems (10). Yet in the one case this information is in a form useful to the human operator while in the other it is not. Similar considerations may apply to other types of skilled performance, as part of the general field known as anticipation.

## Summary

1. This study deals with the situation in which two messages, in different voices, arrive simultaneously from the same spatial direction. Only one of the two messages is to be answered. When the only clue to this message was the usual auditory call sign, 12 *Ss* were able to answer less than 50% of the messages directed to them.

2. When, however, a further unambiguous cue indicated the voice to be answered, another 12 *Ss* were rapidly able to attain an efficiency of about 70%, and since they were showing a practice effect might have improved still further in a longer period. The methods used to indicate the correct voice were (*a*) a visual call sign, and (*b*) previous instructions that one voice was of prior importance. Within the limits of the experiment no difference appeared between the methods.

3. The effect of the unambiguous indication of the correct voice varies with the time at which it is presented. Ten *Ss* to whom it was given towards the end of the message rather than at the beginning (by method (*a*) above) did not do better than with the auditory call sign alone. A further eight *Ss* were used to compare this late presentation of the information directly with the early presentation, and showed an advantage for the latter. Consequently it is clear that it is attentional selection of one voice from the confused noise which is involved, and not simply a need for certainty that the correct response has been chosen from the two available.

4. A final group of 12 *Ss* showed no rise in efficiency, using the auditory call-sign alone, when these call signs were made more distinctive, nor when they were repeated twice before each message. It seems possible that until attentional selection of one voice takes place none of the components of a babel of voices is appreciably effective in producing response, and therefore that auditory call signs can only be relied upon to secure attention

when they do not occur simultaneously with other irrelevant call signs.

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# LOCATION OF OBJECTS IN A VISUAL DISPLAY AS A FUNCTION OF THE NUMBER OF DIMENSIONS ON WHICH THE OBJECTS DIFFER[1]

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An interesting although neglected perceptual problem concerns the relation between the discriminability of an object and the number of dimensions on which it differs from the other objects in the field or group. For example in visual perception, can an object be more quickly located if it differs from the other objects in the field in form, brightness, and hue than if it differs only in form?

There is some evidence that multidimensional differences improve discriminability. An experiment on the Wisconsin Card Sorting Test (2) found that *S*s can sort for a concept based upon the number of symbols on the cards more easily if, for each number, the symbols are also arranged in a specific configuration. In another experiment (4) *S*s responded to different light signals by pulling down corresponding levers. Reaction time was shorter if the levers differed both in form and hue than if they differed in either alone.

There is, however, a lack of any systematic data on the effect of multidimensional differences upon ease of discrimination. Such data are needed, not only to obtain a better understanding of the discriminatory process in general, but also to understand such problems as perceptual level concepts (3) and the role of dominant elements in a stimulus situation.

*Purpose of experiment.*—The aim of the present study was to determine the speed with which objects could be located[2] on a visual display under the following conditions: (*a*) When the various classes of objects on the display differed from one another on only one of the four visual dimensions of form, hue, size, and brightness; and (*b*) when the classes differed from one another on two or on three of these dimensions.

## METHOD

*Materials and apparatus.*—Table 1 shows the seven classes[3] of objects that were used within each of the four visual dimensions. The objects for the form (F) and size (S) dimensions were cut from a uniform dark gray paper, while for hue (H) and brightness (B) 7/8-in. squares of Munsell papers were used. Within the F-dimension size was kept constant by making each of the various forms exactly circumscribable by a circle of 1-in. diameter. This procedure resulted in a series of forms which phenomenally appeared to be approximately equal in size.

For each dimension, one target class (the class to be located by *S*) was chosen: circles, R 5/6, N 8/, and 4/8-in. squares. These particular classes were chosen because a preliminary experiment had demonstrated that these were the most rapidly located from among the other classes in their respective dimensions.

[1] This research was done under Contract N5-ori-166, Task Order I, between the Systems Coordination Division, Naval Research Laboratory, Office of Naval Research, and The Johns Hopkins University. This is Report No. 166-I-149, Project Designation No. NR 784-001, under that contract.

[2] It may be argued whether the ease with which an object can be located in a group is equivalent to discriminability. Discriminability is certainly involved in ease of location, but the latter may involve learned factors in addition.

[3] The term "class" refers to a group of identical symbols. Classes within a dimension differ from each other only on that particular dimension. Thus, the seven classes in the hue dimensions differ only in hue. They are identical in form, size, and brightness.

The display was a 3-ft. square, flat white in color and ruled off into 81 4-in. squares by means of black lines. A ¼-in. circular metal electrode was located at the bottom of each of these small squares and the cards were hung from a small hook at the top. Illumination was provided by an overhead Macbeth Daylight Lamp which was mounted so as to provide an even, shadow-free illumination throughout the display. The display was perpendicular to *S*'s line of sight with the lower border of the display 42 in. from the floor. A sliding panel hid the display from *S*. When a catch was released, the panel dropped giving *S* a full view of the display. The dropping panel also tripped a microswitch which started an electric timer.

A control panel was mounted on the back of the display which provided connections with each of the electrodes on the face. Six telephone jacks could be plugged into these connections so as to obtain any desired pattern among the electrodes. These jacks were connected to a system of relays, which were tripped whenever the stylus touched the appropriate electrode. When all six electrodes had been touched, in any order, the tripped relays completed another circuit which stopped the timer. If the stylus was touched to any electrode other than the selected six, an error was automatically registered.

Although the display provided for a nine by nine matrix of squares, the outer border was never used. Thus only the remaining seven by seven matrix in the center of the display was used. The six target objects were located among these 49 squares according to a series of patterns obtained from a table of random numbers with the restrictions that no two target objects would be adjacent, and at least one of the six would occur in the last row of the matrix. The ten patterns selected for use in the present study were found by experiment to be of equal difficulty. The objects in the remaining classes were randomly distributed among the remaining squares and all unused squares were filled with a blank white cardboard.

TABLE 1

THE SEVEN CLASSES OF OBJECTS USED WITHIN THE FOUR DIMENSIONS

| Classes | Dimensions | | | |
|---|---|---|---|---|
| | Hue* | Form | Brightness* | Size (In.) |
| Target | R 5/6 | Circles | N 8/ | 4/8 |
| 2 | YR 5/6 | Hexagons | N 7/ | 5/8 |
| 3 | Y 5/6 | Diamonds | N 6/ | 6/8 |
| 4 | GY 5/6 | Triangles | N 5/ | 7/8 |
| 5 | G 5/6 | Crosses | N 4/ | 8/8 |
| 6 | BG 5/6 | Stars | N 3/ | 9/8 |
| 7 | B 5/6 | Squares | N 1/ | 10/8 |

* Munsell notation.

*Procedure.*—The *S*'s task was to locate the objects of the target class from among the various classes on the display by touching a stylus to the electrodes beneath the appropriate cards. His score was the time it took him to locate the six target objects. A sample of the target class was mounted to the left of the display and *S* was told to locate the six objects on the display that were the same as the sample. In addition, *E* described the target by saying, ". . . the six circles, the six small light gray circles, etc." Speed of performance was stressed, but *S* was also cautioned against making errors.

*Subjects.*—A total of 60 *S*s was used. They were obtained from among undergraduate students, summer school students, and laboratory assistants at The Johns Hopkins University. All were screened by a shortened version of the Ishihara Color Vision Test (1) to insure that they were color normal. All *S*s were within the age range of 19 through 35 years.

*Design.*—The experiment was divided into two parts. The first step was to determine the speed of location when the classes of objects on the display varied only in one dimension at a time. As a control measure each of these dimensions was tested under three conditions. Under Cond. 1, the target and two other classes were on the display; under Cond. 2, the target and four other classes; and under Cond. 3, the target and six others. The classes were added to the display under the above conditions in the order in which they are listed in Table 1. In all three conditions the total numbers of objects on the display was kept constant at 42 by manipulating the number of objects in the last-added class from 30 for Cond. 1 to six for Cond. 3. The other classes including the target contained six objects.

The sample was 15 males and 15 females. They were randomly assigned by sex to the three conditions. All *S*s had had extensive practice performing similar tasks on the equipment. The four patterns used for the target classes and the order of testing on the four dimensions were systematically varied between *S*s within each condition. Within conditions each *S* was tested once on each of the four dimensions.

In the second part of the experiment the speed in locating the target class was determined when the classes on the display varied on two or on three visual dimensions. The six combinations of F, H, S, and B taken two at a time and the four combinations obtained from taking them three at a time comprised the ten compounded dimensions. Each compounded dimension was

tested under the three conditions described above. Classes were added to the display under the three conditions in the order of their similarity to the target class.

The compounding of dimensions was done by classes through dimensions as given in Table 1. For example, in the compound HFB the target class consisted of red circles with a brightness value of 8/. The next most similar class for this compound was yellow-red hexagons with a brightness of 7/.[4]

An additional 30 *S*s were used on these compounded dimensions (15 males and 15 females). All had received extensive practice on similar tasks. Five males and five females were randomly assigned to each condition. The patterns for the target symbols and the order in which each *S* was tested on the ten compounded dimensions were systematically varied within each condition.

## Results and Discussion

Location times were transformed into three-place logarithms and a modified four-way analysis of variance was performed separately on the data for the four single dimensions and the ten compounds. These analyses showed differences between dimensions and between compounds to be highly significant ($p = .001$). The other main effects, sex and conditions, did not constitute significant sources of variance. Also none of the interactions approached significance. Since the main effect and all interaction variances involving conditions were insignificant, data obtained for each of the three different conditions have been pooled for the rest of the report.

Table 2 presents the mean location times (in logarithms) for each of the four single dimensions and the ten compounded dimensions. The single dimensions and the compounds fall into three distinct groups with respect to speed of location. Location is quickest when the classes differ in HF, H, and HFB. Somewhat slower location is obtained for differences in F, HFS, HB, HS, HSB, and FB and location is considerably slower for FBS, B, FS, S, and BS.

The multidimensional location times obtained by compounding fail to show any consistent advantages over single dimensional location times. HF is the only case where a compound gives an even slightly faster location time than the best of the single dimensions of which it is composed. Compounding differences in H and F with differences in B or S results in slower location than is obtained for differences in either H or F alone. Also the compounding of B and S with H and F does not yield uniform results. B or S when combined with H yields much quicker location than either B or S do singly. However, when they are combined with F there is an interaction. FB is located nearly as fast as F, but FS and FBS are as slow as either B or S is singly.

There is a marked degree of consistency in the location times for the

TABLE 2

Means for the Four Single and the Ten Compounded Dimensions*

| Single Dimensions | Obtained Mean | Compounded Dimensions | Obtained Mean | Predicted Mean |
|---|---|---|---|---|
| H | .678 | HF | .652 | .703 |
| F | .753 | HB | .754 | .758 |
| B | .919 | HS | .772 | .766 |
| S | .942 | FB | .772 | .808 |
| | | BS | .928 | .927 |
| | | FS | .929 | .816 |
| | | HFB | .706 | .757 |
| | | HFS | .766 | .763 |
| | | HSB | .776 | .804 |
| | | FBS | .909 | .842 |

* The means in the table are the mean of the logarithms of location time in seconds. For the single dimensions mean differences of .036 and .049 are significant at the .05 and the .01 levels, respectively. For the compounded dimensions the same levels of significance are reached by mean differences of .031 and .042. In comparing single dimensions with compounded dimensions mean differences of .098 and .133 are significant at approximately the .05 and the .01 levels.

[4] Since hue and brightness were systematically varied, it was not possible to keep the colored objects perfectly matched for saturation in the compounds HB, HFB, and HBS.

compounds. The compounds of two as well as those of three dimensions tend to rank, with respect to location time, in the same order as do the single dimensions of which they are composed. For the single dimensions, H is significantly faster than F, and both H and F are significantly faster than B or S. With the exception of the reversal between FS and BS, the rank ordering of the compounds corresponds to an average of the location times of the single dimensions involved.

A fair approximation to the location times of the different compounds is obtained by using a weighted geometric mean of the location times of the component single dimensions. The predicted values in Table 2 were obtained by a weighted arithmetic averaging of the log location times of the single dimensions. For this average the fastest dimension involved was given a weight of two and the remaining dimensions a weight of one. The correspondence between the obtained and predicted values is reasonable. It is quite reasonable when we consider that the location times for the single dimensions were obtained from one set of *S*s, and those for the compounded dimensions from another set of *S*s. In only two compounds is the obtained location time considerably higher than the predicted location time, and these two involve the interaction between F and S mentioned above. Even with these two erratic cases, however, the rank order correlation between the obtained and predicted location times is .94.

Thus it seems safe to conclude that the location time for objects which differ from other objects in more than one dimension will be some value between those obtained for each of the two or more dimensions when they are located from other objects differing in only that dimension. The only serious exception to this generalization from the present data is the case where two dimensions combine in such a way as to make the judgment on either dimension more difficult.

One point needs further comment. In the present study *S* could have located the target objects for the compounds on the basis of differences on any one of the compounding dimensions. For example, in the compound FBS the target objects could have been correctly located solely on the basis of form differences, irrespective of brightness and size. The *S*s were not aware of this, however. Their instruction was to locate the six objects that were the same as the sample. It might be argued that the results obtained for the compounded dimensions were due to the greater complexity of judgment required from *S* by these instructions. Instead of locating only circles, for example, he now had to make sure that they were not only circles but also of a given brightness and size.

This argument seems plausible, but there are two important points against it in the data. If the argument were valid, we would expect that compounds of three dimensions would give slower location times than compounds of two. Also, the compound BS would be more difficult to locate than either B or S singly. Both of these deductions are contradicted by the data.

## Summary

Speed in locating objects on a visual display was investigated under the following conditions: (*a*) When the various classes of objects on the display differed from one another on only one of the four visual dimensions of form, hue, size, and brightness; and (*b*) when the classes differed from one another on two or on three of these dimensions.

For the single dimensions, location time for hue differences was significantly faster than for form differences, and hue and form were both significantly faster than either brightness or size. The location times for the compounds of two and three dimensions were found to correspond to a weighted geometric mean of the single dimensions of which they were composed. Compounds involving both the form and size dimensions were an exception due to an interaction between these two dimensions.

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